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PROCEEDINGS

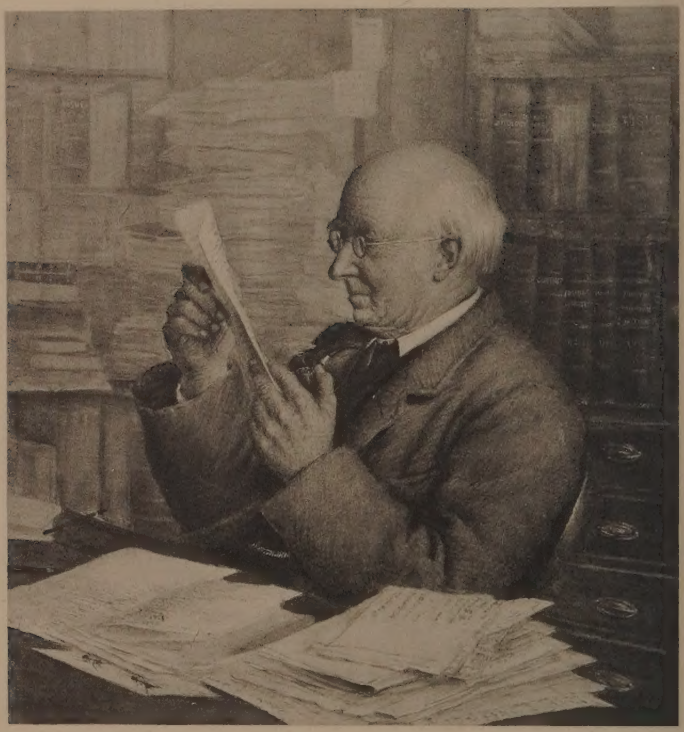
OF THE

LINNEAN SOCIETY OF LONDON.

126TH SESSION.

FROM NOVEMBER 1913 TO JUNE 1914.

LONDON:
PRINTED FOR THE LINNEAN SOCIETY,
BURLINGTON HOUSE, PICCADILLY, W.,
BY TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET.



DR. ALBERT GÜNTHER, F.R.S., P.L.S.

READING THE SWAINSONIAN CORRESPONDENCE

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APPENDIX.

Reprint of G. W. Sleeper's pamphlet: 'Shall we have Common Sense?'

PUBLICATIONS: Session July 1913–July 1914.

Journal, Botany.

Vol. XLI.	No. 283.	15/-
	„ 284.	6/-
Vol. XLII.	„ 285.	20/-
	„ 286.	21/-

Journal, Zoology.

Vol. XXXII.	No. 216.	4/-
	„ 217.	10/-

Transactions, Botany.

Vol. VIII. Part	3	8/-
„	4.	4/-
„	5.	10/-
„	6.	12/-

Transactions, Zoology.

Vol. XVI. Part	2.	21/-
„	3.	12/-
„	4.	30/-

Proceedings, 125th Session, October 1913.

List of [Fellows, Associates, and Foreign Members], Nov. 1913.

PROCEEDINGS

OF THE

LINNEAN SOCIETY OF LONDON.

(ONE HUNDRED AND TWENTY-SIXTH SESSION, 1913–1914.)

November 6th, 1913.

Prof. E. B. Poulton, F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 19th June, 1913, were read and confirmed.

The President announced the deaths of Prof. P. F. A. Ascherson, a Foreign Member, and of Adolphus Henry Kent, an Associate.

The following were proposed as Fellows:—Miss Maude Lina West Cleghorn, Mr. Alfred Eastham, B.Sc. (Toronto), Mr. Arthur Samuel Horne, B.Sc. (Lond.), Mr. Arthur Reginald Horwood, Mr. James John Joicey, Mr. Harold Henry King, and Miss Agnes Whateley Thomson.

Mr. Archibald Joseph Brooks and Mr. Edward Edgar Pescott, F.R.H.S., were elected Fellows.

The Treasurer drew attention to a complete set of the 'Bulletin de la Société Mycologique de France,' on the table, which had been purchased for the Library from the current year's income of the Tagart Fund.

Mr. H. Hamshaw Thomas, M.A., F.G.S., on behalf of himself and Miss Nellie Bancroft, B.Sc., F.L.S., explained the main points of their joint paper, "An Investigation of the Cuticles of some recent and fossil Cycadean Fronds."

Miss Bancroft, Dr. D. H. Scott, and Prof. A. C. Seward engaged in a short discussion upon the paper.

Prof. W. A. Herdman, F.R.S., F.L.S., gave his account of recent work in the S.Y. 'Runa,' entitled "Spolia Runiana II.—Results of the past Season's Dredging." He described the course taken by the yacht off the West Coast of Scotland, and showed a long series of slides displaying the scenery and bird-life of the unfrequented regions visited.

Mr. A. O. Walker contributed some remarks upon the Amphipoda obtained, and Mr. E. Heron-Allen showed additional lantern-slides, and remarked upon the interesting hauls of Foraminifera, both as to their rarity and abundance.

Mr. W. C. Worsdell's series of slides to illustrate Cape plants in their native habitats was postponed.

Dr. A. B. Rendle, F.R.S., F.L.S., showed a bunch of abnormal pears, received from Mr. Ross, of Bournemouth; they exhibited the well-known phenomenon of proliferation in an interesting degree.

Mr. W. C. Worsdell added a few observations on this exhibition.

November 20th, 1913.

Prof. E. B. Poulton, F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 6th November, 1913, were read and confirmed.

Mr. James Lomax and Mr. Richard Francis Towndrow were proposed for the vacant Associateship caused by the death of the late Adolphus Henry Kent.

The President referred to the recent death of our distinguished Fellow, Dr. Alfred Russel Wallace, who cherished a special regard for the Linnean Society, with which he had been more closely associated than with any other scientific society. The Linnean Society had not only published the Darwin-Wallace essay in 1858, but also H. W. Bates's memoir on mimicry—a paper which had greatly inspired and interested Wallace and directly

led to his and Roland Trimen's papers on the same subject in the 'Transactions.' He also read before the Society on June 18, 1896, his last scientific paper—on the Utility of Specific Characters; and no Fellow who was present at the Society's celebration on July 1, 1908, of the 50th anniversary of the joint essay on natural selection, can ever forget the words in which Wallace spoke of his illustrious fellow-worker. The President then proposed a vote of condolence with the deceased naturalist's family, which was tendered by the whole of the Fellows present rising in their places.

The President then spoke of the Hooker Lecture Fund, to which the Fellows had liberally subscribed, the amount of £300 being already invested as a fund to enable the Society to invite distinguished foreigners, both from America and Europe, to deliver lectures on similar work to that achieved by Sir Joseph Hooker. He therefore invited further contributions.

Mr. H. J. Elwes, F.R.S., F.L.S., then delivered his lecture on "The Travels of Sir Joseph Hooker in the Sikkim Himalaya," which was illustrated by specimens, drawings, maps, and lantern-slides.

December 4th, 1913.

Prof. E. B. Poulton, F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 20th November, 1913, were read and confirmed.

Mr. Loftus St. George Byne, M.Sc., and Mr. Gerald Walter Erskine Loder, were proposed as Fellows.

Miss Maude Lina West Cleghorn, Mr. Alfred Eastham, B.Sc. (Toronto), Mr. Arthur Samuel Horne, B.Sc. (Lond.), Mr. Arthur Reginald Horwood, Mr. James John Joicey, Mr. Harold Henry King, and Miss Agnes Whateley Thomson, were elected Fellows.

Prof. John Percival, F.L.S., exhibited specimens of wild wheat, *Triticum dicoccoides*, Koern., from Mount Hermon, Palestine, and gave an account of his experiments in cultivating various forms of wheat from all available sources.

Dr. Gates, Dr. Otto Stapf, and Mr. Frederick J. Hanbury contributed further remarks on the exhibition.

This was followed by a paper entitled :—"On a Collection of Non-Marine Mollusca from the Southern Sudân. By Jane Longstaff, F.L.S. With Descriptions of Three New Species by H. B. Preston, F.Z.S.; and Notes on *Veronicella nilotica*, Cockerell, by G. C. Robson, B.A."

The paper, which was illustrated by lantern-slides and a profuse display of specimens, was discussed by Prof. G. C. Bourne, Sec.L.S., and Dr. Longstaff.

Before explaining the salient features of his own paper, Mr. A. S. Horne, B.Sc., alluded to an exhibition by Messrs. James Veitch & Sons, of the "cross-grafted plants," which had been shown at the International Horticultural Show in London in 1912, the following being the plants now on view :—*Athrotaxis Doniana* grafted upon *Cryptomeria elegans*, *Aucuba japonica* on *Garrya elliptica*, *Benthamia fragifera* on *Cornus Mas*, *Cotoneaster pannosa* on *Pyrus Aucuparia*, *Crataegus Oxyacantha* on *Cotoneaster Simonsii* and on *Raphiolepis japonica*, *Crataegus Pyracantha* on *Photinia Benthamiana* and on the quince, *Cupressus Lawsoniana* and its variety *Allumii* on *Thuya occidentalis*, *Cytisus supinus* on *Laburnum vulgare*, *Eriobotrya japonica* on the quince and on *Raphiolepis japonica*, *Hamamelis* sp. on *Parriotia persica*, *Hippophaë Rhamnoides* on *Elæagnus aurea* var. *picta*, *Laburnum vulgare* on *Cytisus præcox*, *Libocedrus macrolepis* on *Thuya occidentalis*, *Ligustrum ovalifolium* var. *elegantissimum* on *Olea fragrans*, *Olea fragrans* on *Osmanthus ilicifolius*, *Osmanthus ilicifolius* on *Ligustrum ovalifolium*, on *Olea fragrans* and on *Phillyrea ilicifolia*, with its variegated form on *Olea fragrans* and on *Phillyrea ilicifolia*, *Osmanthus rotundifolius* on *Phillyrea ilicifolia*, the latter on *Ligustrum ovalifolium*, *Osmanthus rotundifolius* on *Phillyrea ilicifolia*, the latter also on *Ligustrum ovalifolium*, *Phillyrea Vilmoriniana* on *Olea fragrans*, *Photinia ovata* on the quince, *Photinia serrulata* on *Cotoneaster Simonsii* and on the quince, *Pyrus Aucuparia* on *Cotoneaster Simonsii*, the quince (*Pyrus japonica*) on *Crataegus Pyracantha*, *Raphiolepis japonica* on *Cotoneaster Simonsii*, on *Crataegus Lalandii*, and on *Crataegus Pyracantha*, *Rubus bambusarum* on a rose, *Stranvæsia undulata* on *Crataegus Oxyacantha* and on the quince, *Sycopsis sinensis* on *Hamamelis virginica*, *Thuya orientalis* on *Thuiopsis borealis*, and the latter worked on the former reciprocally. In all, 57 plants were shown.

Mr. Horne then gave an outline of his work during the past eight years which was embodied in his paper :—"A Contribution to the Study of the Evolution of the Flower; with special reference to the Hamamelidaceæ, Caprifoliaceæ, and Cornaceæ." (Communicated by Prof. J. Bretland Farmer, F.R.S., F.L.S.)

Dr. Rendle, Dr. Gates, and Dr. Otto Stapf spoke on various points which had been raised by the author.

December 18th, 1913.

Prof. E. B. Poulton, F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 4th December, 1913, were read and confirmed.

Mr. John William Haigh Johnson, B.Sc., was admitted a Fellow.

The names of Mr. James Lomax and Mr. Richard Francis Towndrow were submitted to a ballot for the vacant Botanical Associateship. The ballot having been closed, the President nominated Dr. G. B. Longstaff, Dr. R. R. Gates, and Mr. E. G. Baker, scrutineers; these having examined the Ballot-papers and cast up the votes, reported to the President, who thereupon declared that a majority of votes had been given in favour of Mr. Lomax, but short of the requisite two-thirds majority.

The name of Mr. James Lomax was then submitted to a second ballot, and the President having counted the votes declared Mr. Lomax to be duly elected an Associate.

Dr. Otto Staff, F.R.S., Sec.L.S., then showed and commented on a ball of raw rubber, which had been picked up on the seashore of St. Helens, Isle of Wight. The long exposure to sea-water had altered the aspect of the ball to such an extent, that the true nature of the object was discernible with difficulty.

Mr. John Parkin, M.A., F.L.S., then gave a summary of his paper on "The Evolution of the Inflorescence." He stated that the evolution of all types of inflorescences is to be traced from the solitary terminal flower.

The first cluster to arise is a cymose group of three flowers (a simple dichasium). This comes about through the pushing-out of floral shoots from the leaf-axils just below the terminal flower.

Two somewhat opposing tendencies now reveal themselves:—

(i.) Either the secondary floral shoots may branch repeatedly, giving rise to the compound or continuous dichasium. From this sympodial cymes etc. can be derived.

(ii.) Or the number of these secondary shoots may be increased. This eventually leads to the formation of true racemose inflorescences.

Solitary axillary flowers are shown to arise in three different ways, and are all capable of being derived from the solitary terminal flower.

Finally, attention is drawn to a little-recognised form of flower-cluster, which is characterised by the main axis, after emitting flowers laterally, continuing its growth vegetatively.

The term *intercalary* is proposed for such. The origin of these inflorescences is indicated, and their further evolution into *pseudo-terminal* inflorescences shown.

A discussion ensued in which the following engaged: Dr. A. B. Rendle, Mr. F. N. Williams, Dr. D. H. Scott, Mr. W. C. Worsdell, Prof. F. W. Oliver, and Dr. O. Stapf, the author briefly replying.

Mr. C. E. Salmon, F.L.S., then read a note on "*Hypericum Desetangsii*, Lamotte, in Britain," illustrating his remarks by a series of specimens of that species and its nearest allies, together with a lantern-slide.

He stated that in 1893 the late Mr. T. Hilton, of Brighton, collected what he considered to be *H. dubium*, Leers, in the vicinity of Lewes. Some years after, the specimen came into the author's hands and was seen not to be the usual plant so named.

Various causes prevented him from visiting the locality at the proper season until the present year, when good examples were examined on the spot and afterwards more minutely at home. A drawing was made of the plant when alive, and a lantern-slide made from the plate which appeared in the 'Journal of Botany' for November 1913, was now shown with Mr. Britten's kind permission.

It appears that the Lewes plant must be placed under the species published by Lamotte (in Bull. Soc. Bot. Fr. xxi. 121) in 1874, as *H. Desetangsii*, and further elaborated, in the same journal, by Bonnet in 1878.

It may be roughly distinguished from *H. perforatum*—of which it has the golden-yellow flowers—by its 4-angled stem; from *H. tetrapterum* by the colour and size of its flowers, and from *H. quadrangulum* (*H. dubium*) by its dotted leaves and narrower sepals. These are main distinctions; finer ones exist.

In the upper leaves of *H. Desetangsii* the main veins only are translucent and the whole leaf is pellucidly-punctate; in *H. quadrangulum* the veins anastomose and all are translucent; pellucid dots are somewhat rare. When they do occur the specimen is referable to the var. *punctatum*, Schinz, but no examples named with authority have been seen.

Bonnet divided *Hypericum Desetangsii* into *α. genuinum* and *β. imperforatum*, but, so far, the latter has not been seen from any British localities. The *genuinum* form grows, as mentioned, near Lewes, and dried specimens from Richmond, Yorkshire, collected by R. B. Bowman, and from near Caton, W. Lancashire, gathered by J. A. Wheldon, have been seen. Dr. Thellung reported it from England in 1912, and it was elicited that the example seen by him was found by Prof. Dr. H. Schinz in 1903 growing spontaneously in the garden of the Rev. Mr. Hooper, at Camborne, Cornwall. Abroad, it has been found in eastern, northern,

and central France, Belgium, Germany, Spain, Italy, Switzerland, and Austria.

Some may contend that *H. Desetangsii* is a hybrid form of *H. quadrangulum* crossed with *H. perforatum* (*H. tetrapterum* is even less likely), but if that were the case, some of the more minute characters of *perforatum* would be present, such as the peculiarly-shaped pellucid dots, long style, absence of membranous margin to leaf, etc.

It appears likely that the extreme form of *H. quadrangulum* with very broad, obtuse, entire sepals is far from common in Britain, our usual plant having these longer and narrower and the tip often eroded or denticulate. Dr. Schinz has described a variety *erosum*, and this, very likely, is our more frequent form. Babington's var. *maculatum* (now var. *Babingtonii*) is probably the same.

As regards the name "*quadrangulum*, L.," this has been used, both on the Continent and in Britain, by various writers to denote either the large-flowered non-dotted-leaved plant or the smaller-flowered dotted-leaved species in a most confusing manner. An examination of the Linnean Herbarium shows that there is some reason for this. Two sheets are pinned together, both labelled "13," which refers to *H. quadrangulum* in 'Species Plantarum,' ed. 1. One of the specimens is *H. tetrapterum*, and upon this sheet Smith has written "*quadrangulum* origiual sp." The other example is labelled by Linnæus "*quadrangulare*" (altered by Smith to "*quadrangulum*," who also added "*dubium*?"), and it is the plant with large flowers, non-dotted leaves, and short obtuse sepals.

Mr. G. C. Druce and Dr. Stapf commented on the exhibition, and Mr. Salmon replied.

The paper by Mr. J. Davidson, M.Sc., F.E.S., on "The Mouth-parts and Mechanism of Sucking in *Schizoneura lanigera*" (communicated by Prof. A. D. Imms, F.L.S.) was postponed.

January 15th, 1914.

Prof. E. B. Poulton, F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 18th December, 1913, were read and confirmed.

Mr. James John Joicey and Mr. Alleyne Leechman were admitted Fellows.

Mr. Thomas Buxton Hardie, Mr. Eric Marsden Jones, Mr. John Kitching, Mr. John Ramsbottom, M.A. (Cantab.), and Mr. Herbert Fuller Wernham, B.Sc. (Lond.), were proposed as Fellows.

Mr. Loftus St. George Byne, M.Sc., and Mr. Gerald Walter Erskine Loder, J.P., D.L., M.A. (Cantab.), LL.B., were elected Fellows.

The Treasurer exhibited a copy of 'Fauna Hawaiiensis,' bound in four volumes, recently acquired for the Society out of the income of the Tagart Bequest.

Mrs. Henshaw gave an account of the journey into the interior of Vancouver Island, made by Mr. Henshaw and herself, illustrated with a series of photographs of the scenery and the more striking plants.

A discussion followed, Dr. Rendle, Dr. C. E. Moss, Mr. A. H. Maude, Dr Stapf, and Dr. Young taking part in it, Mrs. Henshaw replying.

The second paper was read by Mr. H. A. Baylis, B.A. (Oxon.), entitled "Some Observations on the Tentacles of *Blennius gattorugine*," which was communicated by Prof. G. C. Bourne, F.R.S., Sec.L.S.

Prof. G. C. Bourne and the President contributed further remarks, to which the author replied.

Mr. G. Claridge Druce, M.A., F.L.S., read a paper on a Marsh Orchis for which he proposed the name *Orchis prætermissa* (a Latin description being given), and contrasted it with the true flesh-coloured *O. incarnata* of Linnæus, as described by C. B. Clarke in Journ. Linn. Soc. xix. (1881) 206, showing how it differed from that plant in the shape of the flowers and in other characters. *O. prætermissa* is the crimson-flowered plant which has a wide distribution through south and central England. A painting of it has been executed by Miss Trower from a seedling raised by Mr. B. Savile Ogle, who had collected the parent plant before 1903 on the borders of Berkshire and Hants. The seedlings obtained from it resembled each other and the parent in all the stages of their growth. The parent was figured as *O. incarnata* in the 'Report of the Ashmolean Natural History Society of Oxfordshire' for 1904.

Mr. Druce himself collected the plants in Nottinghamshire in 1878, in Oxfordshire in 1882, in Berkshire and in Norfolk.

Mr. P. M. Hall and Mr. R. B. Ullman, who have studied the Orchids from round Winchester with great assiduity, expressed independently the opinion that it was a distinct species (a note on it appears in the curtailed 'Report of the Winchester College Nat. Hist. Society'); they found it abundantly in Hampshire. A photograph by Mr. Bedford showed that it occurred near Lewes in Sussex.

Reference was made to another and as yet undescribed form from the coast sands of Britain, as well as to a northern plant, but these await further investigation.

Mr. Druce stated that he has as yet been unable to find any description or figure of his plant in British or European works.

Mr. Druce then read a note on Article 45 of the Vienna Rules, contending that the change of *Limonium* to *Statice* was either unnecessary, or, if persisted in, might open the door to other changes in nomenclature. His point was that the Linnean genus *Statice* was made up of the two Tournefortian genera *Statice* and *Limonium*; that in his 'Genera Plantarum' Linnæus alludes to and gives descriptions of both these under *Statice*, placing first the Sea Thrifts under *Statice*. Miller and Hill correctly chose *Limonium* to represent the Sea Lavenders, leaving *Statice*, Linn., to denote the Sea Thrifts. A list of genera, in which the retained name represents a minority of plants in the genus, had been drawn up, but many of these were correct on the same evidence as that of the example given.

He further suggested that, in order to clear up some apparent ambiguity in Art. 45, the words "has no retrospective action" might be added to it.

Mr. Druce's third paper paper gave a list of the names of genera defined by Miller, Abridgement of the 'Gardener's Dictionary' of 1754, which (as wholly Tournefortian) were not included in the 'Index Kewensis': thus Miller may have to be cited for many of those genera, instead of later authors such as Adanson, Moench, Gilibert, etc.

To this was appended a note concerning John Hill's 'British Herbal' of 1756, with its accidental binomials, which in some cases preceded those cited from Garsault's 'Materia Medica' of 1768, which have been inserted in the fourth supplement to the 'Index Kewensis.'

These short papers produced a brisk discussion, the following speakers engaging in it: Mr. James Britten, Dr. C. E. Moss, Mr. Pugsley (visitor), Mr. C. E. Salmon, Mr. F. N. Williams, Dr. A. B. Rendle, the General Secretary, and Mr. A. J. Wilmott, Mr. Druce replying on certain points.

February 5th, 1914.

Prof. E. B. Poulton, F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 15th January, 1914, were read and confirmed.

Mr. Arthur Samuel Horne, B.Sc. (Lond.), and Mr. Gerald Walter Erskine Loder, J.P., D.L., M.A. (Cantab.), LL.B., were admitted Fellows.

The PRESIDENT adverted to the death on the 1st February of Dr. ALBERT KARL LUDWIG GOTTHILF GÜNTHER, President of the Society from 1896 to 1900, and suggested that a vote of condolence be sent to his family, which was approved by all present rising in their places.

The author being abroad, the Secretary for Zoology then read an abstract of a paper by Mr. JAMES DAVIDSON, M.Sc., F.E.S., entitled "The Mouth parts and Mechanism of Suction in *Schizoneura laniyera*, Hausm.," and communicated by Dr. A. D. IMMS, F.L.S.

The General Secretary, on behalf of Miss WING, exhibited a series of coloured drawings of British insects by her late brother, Mr. WILLIAM E. WING, which are now available for disposal.

The Secretary for Botany then read a paper, "The Vegetation of White Island, New Zealand," by Mr. W. R. B. OLIVER, communicated by Dr. L. COCKAYNE, F.R.S., F.L.S., and illustrated it by herbarium specimens, and lantern-slides showing the island, with the lake emitting steam and hydrochloric acid, the effect of which upon the flora was very marked. A list of twelve plants was appended.

Mr. W. C. WORSDELL, F.L.S., exhibited a series of 93 lantern-slides of Cape plants, mostly in their native habitats; most of the photographs had been taken by Prof. W. T. SAXTON.

A discussion followed in which Mr. N. E. Brown, Dr. Stapf, and Mr. T. A. Dymes took part, Mr. Worsdell briefly replying.

The last paper was by Mr. WALTER E. COLLINGE, M.Sc., F.L.S., entitled "On the Range of Variation of the Oral Appendages in some Terrestrial Isopods." The author had been unexpectedly prevented from reading the paper himself, but the conclusion of the paper was read by the Secretary.

The Rev. T. R. R. Stebbing contributed a few additional remarks.

February 19th, 1914.

Prof. E. B. POULTON, F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 5th February, 1914 were read and confirmed.

Mr. Edward Ashdown Bunyard, and Miss Ellen Marion Delf, B.A. (Dubl.), D.Sc. (Lond.), were proposed as Fellows.

Mr. Thomas Buxton Hardie, Mr. Eric Marsden Jones, Mr. John Kitching, Mr. John Ramsbottom, M.A. (Cantab.), and Mr. Herbert Fuller Wernham, B.Sc. (Lond.), were elected Fellows.

Dr. John P. Lotsy, of Haarlem, read a paper entitled "On the Origin of Species by Crossing," illustrating his points by diagrams, lantern-slides, and dried specimens. (Abstract, pp. 73–89.)

A discussion followed, in which the following speakers took part:—Prof. W. Bateson, Prof. J. B. Farmer, Mr. P. L. de Vilmorin, Dr. R. R. Gates, Prof. F. Keeble, Prof. A. Dendy, Sir Francis Darwin, Dr. A. B. Rendle, Prof. E. W. MacBride, F.R.S. (visitor), Prof. F. E. Weiss, Major C. C. Hurst, Mr. Arthur W. Sutton, Dr. O. Stapf, Miss E. R. Saunders, and the President, the author briefly replying. (Abstracts, pp. 89–98.)

March 5th, 1914.

Prof. E. B. Poulton, F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 19th February, 1914, were read and confirmed.

Miss Agnes Whateley Thomson and Mr. Arthur Reginald Horwood were admitted Fellows.

Major Frank Wall. I.M.S., C.M.Z.S., was proposed as a Fellow, and Prof. Robert Chodat of Geneva, and Prof. Richard Wettstein Ritter von Westersheim as Foreign Members.

A paper by the Misses Katharine Foot and E. C. Strobell, on "Results of crossing *Euschistus variolarius* and *E. servus*, with reference to the Inheritance of an exclusively Male Character," was explained by Miss Foot, by the aid of lantern-slides.

The President opened a discussion in which Dr. R. R. Gates, Prof. Ramsay Wright (visitor), and Prof. Bourne, Sec.L.S., took part.

The second paper was by Mr. C. F. M. Swynnerton, entitled "Short Cuts by Birds to Nectaries," illustrated by lantern-slides from photographs and drawings by the author.

Dr. Stapf, Sec.L.S., added a few remarks, and the author stated that he hoped to continue his observations on his return to Africa.

The following four papers, communicated by Prof. J. Stanley Gardiner, F.R.S., F.L.S., on collections made by the Percy Sladen Expedition to the Indian Ocean, were formally read in title:—

Mr. Ch. Kerremans.—Buprestidæ.
Lt.-Col. Winn Sampson.—Platypodidæ and Ipidæ from the Seychelles.
Dr. G. Enderlein.—Scatopsidæ and Simuliidæ.
Mr. C. G. Lamb.—Heteroneuridæ—Milichiidæ.

March 19th, 1914.

Prof. E. B. Poulton, F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 5th March, 1914, were read and confirmed.

Mr. Francis Druce, M.A., the Rev. Edward Francis Linton, M.A. (Oxon.), Mrs. Mary Newman Tremearne, L.L.A., and Mr. Mudumalakurti Venkata Rau, M.A. (Madras), were proposed as Fellows.

Mr. Edward Ashdown Bunyard, and Miss Ellen Marion Delf, B.A. (Dubl.), D.Sc. (Lond.), were elected Fellows.

Dr. E. F. Armstrong, F.C.S., read a paper entitled "The Bearing of Chemical Facts on Genetical Constitution," which was communicated by Prof. F. Keeble, Sc.D., F.L.S. The following abstract was furnished by the author:—

(1) The Relation of Enzymes to Colour Inheritance in Plants.—The known genetical relation obtaining between the colour-series of *Primula sinensis* as typified by the colour series:—

Dominant White—Magenta—Red—Blue—
Recessive White.

The universal presence of oxydase in all the members of this series. The demonstration and delineation of oxydase in plant-tissues by means of the benzidine reagent and similar artificial chromogens. The apparent failure of dominant white flowers to show the oxydase reaction is due to inhibition. The inhibitor may be removed by means of hydrogen cyanide, carbon dioxide, absolute alcohol, and certain other reagents. The presence of oxydase can be then demonstrated.

(2) The Nature of Oxydases.—The present position of our knowledge based on a review of the work of Bach and Chodat,

Palladin, Gortner, etc. The manifestation of oxydase reactions in solutions of different degrees of hydration—the possible bearing of these facts on the course of chemical events in plant cells.

(3) The Anthocyan Pigments of Plants and their Mode of Formation.—The work of A. G. Perkin, Wheldale, Willstätter, etc.

The Glucoside nature of the antecedents of anthocyanin. The stages in the formation of pigment :—

Glucoside + enzyme ⟶ Sugar + chromogen.
Chromogen + enzyme + peroxide ⟶ pigment.

(4) Other Plant Pigments.—The action of Oxydase on Amino Acid and on phenolic bodies in the presence of Amino Acids.

Formation of pigments from glucosides (*e. g.* Arbutin) in this manner. Biological synthesis of the principles of both colour and odour of the ripe fruit.

(5) An hypothesis as to the relation between the several members of an epistatic series of pigments.

Mendelian interpretation of an epistatic colour-series.

Parallel behaviour of certain phenolic substances, *e. g.* methyquinol, which do not give a colour-reaction with oxydase *per se*, but only when another compound, such as benzidine, is present to transmit oxygen to them.

A discussion followed in which the participants were: Miss Wheldale (visitor), Prof. W. Bateson, Sir E. Ray Lankester, and Prof. H. E. Armstrong (visitor), Dr. E. F. Armstrong briefly replying.

April 2nd, 1914.

Dr. A. Smith Woodward, F.R.S., Vice-President, in the Chair.

The Minutes of the General Meeting of the 19th March, 1914, were read and confirmed.

Miss Sarah Martha Baker, D.Sc. (Lond.), Mr. Charles John Bond, and Mr. Stuart Hogg, were proposed as Fellows.

Major Frank Wall, I.M.S., was elected a Fellow.

The following Auditors for the Society's Accounts were nominated by the Council and, by show of hands, elected: for the Council, Dr. W. T. Calman and Prof. Percy Groom; for the Fellows, Mr. James Groves and Mr. Alfred W. Oke.

Miss Ida M. Hayward, F.L.S., exhibited a series of 46 Australian plants, found growing on the banks of the River Tweed and its tributary the Gala, with a large collection of Australian seeds, the whole obtained from Australian wool. Many of the species had been determined by Dr. A. Thellung, Dr. Josef Murr, and other specialists, the following being pointed out as of exceptional interest:—*Acæna Sanguisorbæ*, Vahl, *Lepidium peregrinum*, Thell., *Millotia depauperata*, Stapf, *Centipeda orbicularis*, Lour., *Brachycome collina*, Benth., *Stuartina Muelleri*, Sond., *Toxacanthus Muelleri*, Benth., *Danthonia semiannularis*, R. Br., *Pteris aquilina*, Linn., var. *esculenta*.

Mr. G. C. Druce, Dr. A. B. Rendle, Dr. Stapf, the Rev. T. R. R. Stebbing, and the Vice-President in the Chair, engaged in a discussion upon this exhibition, which was further enhanced by a series of lantern-slides.

Mr. W. C. Worsdell, F.L.S., then gave a second series of 67 lantern-slides of Cape plants, mostly in their native habits.

These were commented upon by Mr. P. F. Fyson, Mr. N. E. Brown, and Dr. A. B. Rendle; Mr. Worsdell replying.

Mr. Clement Reid, F.R.S., showed a lantern-slide of photographs from seeds of a new species of *Corema* (*C. intermedia*) from the Pliocene Cromer Forest Bed. The same had also been found in a similar deposit in the Netherlands, at Tegelen.

Mr. W. Rushton gave a summary of his paper, "Structure of the Wood of Himalayan Junipers," which was communicated by Prof. Percy Groom, D.Sc., F.L.S. The four species which were investigated were:—*Juniperus communis*, Linn., *J. pseudo-sabina*, Fisch. & Mey. [syn. *J. Wallichiana*, Hook. f. & Thoms.], *J. recurva*, Buch.-Ham., and *J. macropoda*, Boiss.

Mr. W. B. Turrill gave an account of his paper, "A Contribution to the Flora of Fiji," which was communicated by Dr. Otto Stapf, F.R.S., F.L.S., and illustrated by a series of views of the main island of Viti Levu, taken by Sir Everard im Thurn, K.C.M.G.

Mr. H. N. Ridley and Dr. Stapf added some further observations, and the author briefly replied.

Mr. R. Allen Rolfe, A.L.S., exhibited a series of coloured drawings of five hybrid Ophryses, raised by Mr. Fernand Denis, Balaruc-les-Bains, France, from *Ophrys tenthredinifera*, Willd., crossed with the pollen of *O. aranifera*, Huds.; together with the two parents. This was believed to be the first hybrid *Ophrys* raised artificially, and it proved the origin of a natural hybrid that has been recorded from three localities in Italy, and is known under the names of *O. Grampinii*, Cortesi, and *O. etrusca*, Asch. & Gräbn. The hybrids varied somewhat between themselves, but all showed an unmistakable combination of the characters of the two parents, particularly in the colour and markings of the lip, and in the peculiar combination of rose and green in the sepals and petals. Mr. Denis has a batch of some forty seedlings in flower or bud. At least eighteen natural hybrid Ophryses have been recorded, and Mr. Rolfe believed there were others. He would be greatly obliged to anyone who would send him examples, as he is studying them.

Dr. A. B. Rendle referred to the importance of these observations and experiments as tending to clear up the doubts as to the affinities of many of these allied forms, so troublesome from a taxonomic point of view.

The two following papers were read in title:—Prof. Charles Chilton, LL.D., F.L.S., "A new Amphipodan Genus and Species (Family Dexaminidæ) from New Zealand"; and Prof. E. B. Poulton, F.R.S., P.L.S., "Mr. W. A. Lamborn's Breeding Experiments upon *Acræa encedon* (Linn.), in the Lagos District of West Africa, 1910–1912."

May 7th, 1914.

Prof. E. B. Poulton, F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 2nd April, 1914, were read and confirmed.

Mr. Eric Marsden Jones and Dr. Ellen Marion Delf were admitted Fellows.

The Rev. John Robert Lewis Kingon, M.A., Mr. Robert Ray, B.Sc. (St. And. & Cape), and Miss Clotilde von Wyss were proposed as Fellows.

Mr. Francis Druce, M.A., F.R.G.S., the Rev. Edward Francis Linton, M.A. (Oxon.), Mrs. Mary Newman Tremearne, L.L.A.,

and Mr. Mudumalakurti Venkata Rau, M.A. (Madras), were elected Fellows, and Prof. Robert Chodat and Prof. Richard Wettstein, Ritter von Westersheim, were elected Foreign Members.

The President announced that the Council had decided to award the Linnean Medal at the forthcoming Anniversary Meeting to Prof. Otto Bütschli, of Heidelberg.

Mr. H. N. Ridley, C.M.G., F.R.S., F.L.S., then gave an account of "The Botany of the Utakwa Expedition, Dutch New Guinea," which had been worked up by various botanists. He stated that the extensive collection of plants made by Mr. C. B. Kloss during Mr. Wollaston's expedition to Mt. Carstensz, Dutch New Guinea, in 1912–13, is the most important collection of New Guinea plants brought to this country.

Mr. A. F. R. Wollaston exhibited a series of photographs of the scenery and vegetation taken during the expedition, and Major Gage, Dr. A. B. Rendle, Mr. E. G. Baker, and Dr. Otto Stapf discussed the paper.

Prof. G. C. Bourne, Sec.L.S., gave an abstract of a short paper by Mr. Geoffrey W. Smith, F.L.S., "The Genus *Lernæodiscus*, F. Müller, 1862."

Major Gage then showed lantern-slides, (1) of views in the Botanic Gardens at Sibpur, Calcutta, and (2) the Government Cinchona plantations in Sikkim.

Dr. Otto Stapf, Sec.L.S., explained the chief points of a brief paper by Dr. J. C. Willis, F.L.S., "A new Natural Order of Flowering Plants: Tristichaceæ," separated from Podostemaceæ.

There was exhibited for Mr. James Hornell, F.L.S., "The forced or cultural production of free, spherical Pearls; a preliminary note on a new method," together with specimens in jars of (1) a dissection of a species of *Unio* having an induced free pearl in the tissues of the anterior adductor muscle, (2) three minute pearls from the mantle of the Indian and Ceylon pearl-oyster, *Margaritifera vulgaris*, Schumacher, and (3) two pieces of the mantle of two other individuals of the same species containing three other free pearls, all produced by artificial stimulus, but the precise method was not disclosed. The minute pearls were shown under a microscope.

Two remaining papers were read in title, namely:—Prof. Charles Chilton, LL.D., F.L.S., "Some Terrestrial Isopoda from New Zealand and Tasmania; with the Description of a new Genus, *Notoniscus*"; and Mr. G. C. Champion, A.L.S., "Curculionidæ from the Indian Ocean," being a report on material brought home by the Percy Sladen Expedition.

May 25th, 1914.

Anniversary Meeting.

Prof. E. B. Poulton, F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 7th May, 1914, were read and confirmed.

Mrs. Mary Newman Tremearne, L.L.A., was admitted a Fellow.

Mr. Walter Henry Charter, Mrs. Edith Jane Durrant (Mrs. Wilfred Durrant), and Mr. Linnæus Eden Hope, were proposed as Fellows.

The Treasurer laid his Annual Statement of Accounts before the Meeting, and explained the various items of receipts and expenditure, and the same was received and adopted upon the motion of the President (see pp. 19–21).

The General Secretary then laid his Annual Report before the Meeting, thus:—

Since the last Anniversary 17 Fellows have died, or their deaths been ascertained:—

Tempest Anderson, M.D., D.Sc.	Thomas Hawkes Russell.
The Rt. Hon. John Lubbock, Baron Avebury, P.C.	Philip Lutley Sclater, D.Sc., Ph.D.
Arthur Ellson Davies, Ph.D.	Frederick Granville Sinclair, M.A.
Peter Ewing.	Martin John Sutton.
Albert Karl Ludwig Gotthilf Günther, M.D., Ph.D.	Arthur W. Walker.
Allen James Hewitt.	Alfred Russel Wallace, O.M., LL.D.
Sir John Murray, K.C.B., Sc.D.	William West.
George Henry Parke.	The Rev. William Williams, D.D.
The Rev. John Robinson Porter, M.A.	

Also 3 Foreign Members:—

Prof. Dr. Paul Friedrich August Ascherson.
Prof. Carl Chun.
Prof. Philippe Edouard Léon Van Tieghem.

And 1 Associate:—

Adolphus Henry Kent, B.A.

Fellows withdrawn (9):—

William Francis Cooper, B.A.	Linsdall Richardson.
Alexander Hutton.	Buxton Shillitoe.
John Rudd Leeson, M.D.	John Frederic Waby.
James Paton.	Prof. Richard Henry Yapp, M.A.
William Joseph Rainbow.	

Fellows whose names were removed from the List by order of the Council:—

Robert Carlisle Appleton.	Paul Rivalz Dupont.
Henry Caracciolo.	Charles Aubrey Ealand.

Fellows elected 27, of which 25 have qualified up to the present; also 2 Foreign Members and 1 Associate elected.

The Librarian's Report was as follows:—

During the past year there have been received as donations from private individuals 101 volumes and 165 pamphlets.

From the various Universities, Academies, and Scientific Societies there have been received in exchange, and otherwise, 367 volumes and 34 detached parts, besides 93 volumes and 13 parts obtained in exchange, and as donations from the editors and proprietors of independent periodicals.

The Council, at the recommendation of the Library Committee, have sanctioned the purchase of 246 volumes and 53 parts of important works.

The total additions to the Library are therefore 807 volumes and 265 separate parts.

The number of books bound during the year is as follows:—In full-morocco 3, in half-morocco 270, in half-calf 3, in full-cloth 327, in vellum 24, in buckram 32, in boards and half-cloth 12; relabelled (half-morocco and cloth back) 18. Total 689 volumes.

The General Secretary having read the Bye-Laws governing the elections, the President opened the business of the day,

Treasurer's Accounts for the Year ended April 30th, 1914.

(Presented at the Anniversary Meeting, May 25th, 1914.)

Receipts and Payments of the Linnean Society from May 1st, 1913, to April 30th, 1914.

General Account.

Receipts.	£	s.	d.
Balance at Bankers on 1st May, 1913	306	18	0
Interest on Investments	250	3	6
Admission Fees	156	0	0
Annual Contributions	1614	0	0
Compositions	192	0	0
Sales of Publications :—			
Transactions £136 7 1			
Journals 73 16 1			
Proceedings and Catalogues 4 10 2			
	214	13	4
Sir Frank Crisp.—Donation for purchase of Wallichian Cabinets for presentation to Kew Gardens	50	0	0
Fellows' postal account, deposits	20	15	0
Donations in aid of Publications	200	0	0
Miscellaneous Receipts	17	4	8
	£3021	14	6

Payments.	£	s.	d.
Taxes and Insurance	14	4	6
Repairs and Furniture	90	5	2
Coal, Electric Current, and Gas	40	13	9
Salaries	845	6	5
Library :—			
Books £238 7 3			
Binding 90 19 9			
	329	7	0
Expenses of Publications :—			
Printing £584 5 1			
Illustrations 225 4 10			
Distribution 39 9 1			
	848	19	0
Miscellaneous Printing and Stationery	127	8	11
Petty Expenses (including Tea and Postage) £97 6 5			
Fellows' postal account 7 13 3			
	104	19	8
Linnean Medal	15	0	0
Investment of Compositions	75	19	0
Balance at Bankers, 30th April, 1914 (including £63 14 7, deposits, Fellows' postal account) £525 6 5			
Cash in hand 4 4 8			
	529	11	1
	£3021	14	6

Separate Account.

	£	s.	d.
To Balance at Bankers, May 1st, 1913:—			
Westwood Fund £18 9 8			
Trail Award Fund 11 1 0			
Crisp Award Fund 6 8 2			
Tagart Fund 1 5 4			
Schulze Fund 3 15 6			
Hooker Lecture Fund 13 7			
	41	13	3
,, Contributions towards Publication of Schulze's 'Nomenclator Animalium'	23	12	6
,, Contributions to the Hooker Lecture Fund	27	2	0
,, Linnean Dinner Receipts	29	8	0
,, Grant from General Account to Tagart Fund		16	2
,, Interest on Investments:—			
Westwood Fund £6 19 4			
Trail Award Fund 3 10 0			
Crisp Award Fund 6 6 4			
Tagart Fund 18 11 0			
Hooker Lecture Fund 10 12 6			
	45	19	2
	£168	11	1

	£	s.	d.
By Grant from Westwood Fund for illustrations in Dr. Imms's paper on *Embia major*.....	17	7	6
,, Purchase of £43 3 5 Metropolitan Water Board 3 % "B" Stock (Hooker Lecture Fund)	32	17	1
,, Purchase of Bull. Soc. Mycol. France, Vols. 1–28, 1885–1912 ..... £17 0 0			
,, Purchase of 'Fauna Hawaiiensis' (Tagart Fund) 3 12 6			
	20	12	6
,, Amount remitted to Prof. F. E. Schulze from 'Nomenclator Animalium' Fund	27	8	0
,, Cost of Linnean Dinner.....	29	8	0
,, Balance at Bankers:—			
Westwood Fund £8 1 6			
Trail Award Fund 14 11 0			
Crisp Award Fund 12 14 6			
Hooker Lecture Fund 5 11 0			
	40	18	0
	£168	11	1

Investments on April 30th, 1914.

General Account.

	£	s.	d.			£	s.	d.
Metropolitan Consolidated 3½ per cent. Stock	1150	0	0	@	100½	1155	15	0
Great Indian Peninsula Railway, Annuity Class B	42	1	5	@	20	841	8	4
Forth Bridge Railway 4 per cent. Stock	450	0	0	@	99	445	10	0
Metropolitan Consolidated 3 per cent. Stock	1000	0	0	@	88	880	0	0
India 3 per cent Stock	1175	0	0	@	75¼	884	3	9
Eastern Bengal Railway 4 per cent. Debenture Stock	1000	0	0	@	99	990	0	0
Great Western Railway 4 per cent. Debenture Stock	1000	0	0	@	104	1040	0	0
Midland Railway 2½ per cent. Preference Stock	300	0	0	@	62¾	188	5	0
						£6425	2	1

Separate Account.

	£	s.	d.			£	s.	d.
Metropolitan Water Board 3 per cent. "B" Stock (Westwood Fund)	232	5	0	@	80	185	16	0
Ditto ditto (Hooker Lecture Fund)	393	3	5	@	80	314	10	9
New South Wales 3½ per cent. Stock (Trail Award Fund)	100	0	0	@	92	92	0	0
2½ per cent. Consolidated Stock (Crisp Award Fund)	252	18	0	@	75½	190	18	9
East Indian Railway 3½ per cent. Debenture Stock (Tagart Fund)	530	0	0	@	87	461	2	0
						£1244	7	6

HORACE W. MONCKTON, *Treasurer.*

We have (in conjunction with the Professional Auditor, who certifies as to all details) audited the Accounts of the Society for the year ended 30th April, 1914, and found them correct. We have verified the Investments and Bank Balances.

Dated this 14th May, 1914.

W. B. KEEN, *Chartered Accountant.*

OTTO STAPF,
PERCY GROOM,
W. T. CALMAN,
JAMES GROVES,
ALFRED W. OKE,
} *Auditors.*

and the Fellows present proceeded to vote for the Council and Officers.

The Ballot for the Council having been closed, the President appointed the Rev. T. R. R. Stebbing, Col. H. H. Johnston, and Mr. C. Sillem, Scrutineers; these having examined the Ballot-papers and cast up the Votes, reported to the President, who declared the result as follows:—

Richard Assheton, F.R.S.; Prof. G. C. Bourne, F.R.S.; Dr. W. T. Calman; A. D. Cotton, Esq.; Sir Frank Crisp, Bart.; James Groves, Esq.; Prof. W. A. Herdman, F.R.S.; Dr. B. Daydon Jackson; Prof. E. A. Minchin, F.R.S.; Horace W. Monckton, F.G.S.; Dr. C. E. Moss; Prof. E. B. Poulton, F.R.S.; Dr. A. B. Rendle, F.R.S.; H. N. Ridley, F.R.S.; Miss Edith R. Saunders; Prof. A. C. Seward, F.R.S.; Dr. Otto Stapf, F.R.S.; Miss Ethel N. Thomas, B.Sc.; Comr. J. J. Walker, R.N.; and Dr. A. Smith Woodward, F.R.S.

(The five retiring Councillors were Dr. Percy Groom, Mr. A. W. Hill, Prof. F. Keeble, Prof. F. W. Oliver, and Dr. W. G. Ridewood.)

The Ballot for the Officers having been closed, the President appointed Dr. R. R. Gates, Prof. G. S. Boulger, and Dr. A. P. Young, Scrutineers; these having examined the Ballot-papers and cast up the Votes, reported to the President, who declared the result as follows:—

President: Prof. E. B. Poulton, F.R.S.

Treasurer: Horace W. Monckton, F.G.S.

Secretaries: Dr. B. Daydon Jackson.
Dr. Otto Stapf, F.R.S.
Prof. G. C. Bourne, F.R.S.

The President then delivered his Address, a continuation and completion of his investigation of a remarkable pamphlet by Mr. G. W. Sleeper, with lantern-slides of documents throwing light upon the genesis of the work, which had not been cleared up at the time of his previous Anniversary Address, as follows:—

PRESIDENTIAL ADDRESS, 1914.

When I had the pleasure of addressing you on May 24, 1913, I little thought that the same subject would require another year's intermittent work, and furnish the material for another Anniversary Address. But G. W. Sleeper's booklet, dated 1849, had been in my hands only two months, and the enquiry was still proceeding, when the address was delivered. I had informed the author's son, Mr. J. F. Sleeper, that the registration of the work was not to be found among the copyright records for Massachusetts for the year 1849, that there was no evidence of a copy having been deposited, and that therefore any documentary proofs which he could furnish would be of the utmost importance. His reply, which arrived on June 2nd, a few days after the delivery of the Address, shewed that decisive evidence was in his possession:—

> Bense's contract, signed and sealed by him and dated May 18, 1849, is among my father's papers; there is an entry in his old memo book for 1849, "Bense, pamphlets, $30," under that date; and an MS. lecture, "Life and the Cosmosite," refers to the Origin of Life as delivered in 1847.

Mr. J. F. Sleeper had previously stated that the word "Agnostic" had been introduced by his father, and that it was to be found in the manuscript of a lecture to which he assigned the date 1846.

I wrote asking for the loan of all these documents, in order that they might be examined by experts. Mr. Sleeper replied that, in the absence of the expected registration, they had become so precious that he could only submit them one at a time, and could only despatch the second after the safe return of the first, and so with the rest. This meant loss of time; but the perfectly reasonable conditions could not be resisted, and I still hoped that expert opinion would be unanimous, no further evidence required, and the whole subject brought to a decisive conclusion in a terminal note to the 1913 Address. With this hope, the revised proofs were held back from the press until late in the summer; but by August 13 it was obvious that the enquiry was far from complete, and I therefore returned the proofs to the General Secretary, after adding the brief explanatory paragraph on p. 45.

I fully expected when the proofs were returned that the investigation would be finished in time for a meeting in the autumn session; but, partly because I could not devote myself to this subject continuously, although mainly on account of the

inherent difficulty of pursuing enquiries in another and distant land, it was soon realised that many months must elapse.

The necessary delay has been of special and great advantage, in that it has afforded the opportunity for settling, as I believe finally, the date of certain type used in printing the booklet—a problem which last year appeared to be insoluble.

I do not think that the Fellows of the Linnean Society will criticise too severely the devotion of two Anniversary Addresses to this interesting subject. Had I not seen, in the documentary evidence and the lines of enquiry suggested by it, the probability of a final decisive solution, I should not have hesitated to express my own conclusions as to the authenticity or otherwise of the booklet; although it is not to be expected that the opinion would have received universal or even general approval. On the evidence available a year ago, many thinkers held that the booklet was the outcome of remarkable genius, many that it was the product of remarkable fraud. Whether genius or fraud, it is certainly remarkable, as I fully realised when the title of the address was deliberately chosen:—"A remarkable American Work upon Evolution and the Germ Theory of Disease."

The evidence now brought forward will probably lead the scientific and literary world to an undisputed conclusion—that the work is a fraud; but, when the story is told, I do not doubt that the word "remarkable" will, by general agreement, be allowed to stand.

Much kind help has been received in the course of the investigation, which, indeed, could not otherwise have been carried to a successful issue. All such help is acknowledged in the course of the Address, but I must at the outset specially record my grateful thanks to Mr. Horace Hart, Controller of the Oxford University Press; Mr. J. W. Phinney, Manager of the American Type-founders' Company, Boston, Massachusetts; Sir George Warner, late Keeper of the Manuscripts of the British Museum; Mrs. Frederic Endicott, of Canton, Massachusetts, a daughter of the late Mr. W. Bense; Dr. H. Putnam, Librarian of Congress, Washington; Mr. H. G. Wadlin, Librarian of the Public Library, Boston.

I also desire to thank Mr. F. Madan, Bodley's Librarian, Oxford University, for his great kindness in correcting the proofs of the reproduction in the Appendix, comparing them line for line and word for word with the original booklet. Fellows of the Linnean Society owe to his kindness, and to the skill of the experts of the Oxford University Press, that they will possess an exact reproduction in all essentials of the work stated to be written by G. W. Sleeper and dated 1849.

Mr. J. F. Sleeper has been most kind in supplying documents and answering questions, and I fear that the long correspondence has absorbed much of his time. I regret that I have not been able to accept his conclusions. On p. 44 I have ventured to

suggest the mental condition which led to the fabrication of evidence. The solution offered—that of self-deception—is, I hope, not only the correct one, but also the one which will bring the least distress to my courteous correspondent.

1. *The Type.*

At the time when the revised address for 1913 was sent to press, no clear evidence based on the type had been forthcoming (Proc. Linn. Soc. 1912–13, pp. 40, 41). Some of the founts had been found by Mr. Horace Hart in an undated catalogue of V. and J. Figgins, but their successors had been unable to trace the history. Mr. Hart, on the whole, concluded that the booklet "might very well have been printed in Boston at the date mentioned" (p. 41).

The enquiry has now been carried much further, and has produced results of the highest importance. On July 16, 1913, Mr. Hart wrote the following letter to his friend, the late Mr. Theodore L. De Vinne, of 300 West Seventy-sixth Street, New York:—

I am troubling you with a question about the date of certain types, which will interest you, especially if you are able to make the necessary references to typefounders' catalogues in your own possession.

I have a pamphlet, the titlepage of which is as follows:—

SOME RECENT LECTURES.
By GEO. W. SLEEPER.
Boston: Wm. Bense, Printer, 1849.

I have been assisting . . . . . to fix the date by means of the types, and I have chosen three founts as tests. These are:

(1) Type used for certain headings: Figgins's two-line long primer Monastic No. 2.
(2) Ditto. : two-line brevier Monastic No. 2.
(3) Ditto. : two-line small pica wide Latin.

The point I would submit to you is,—Can you say with any certainty that 1, 2, or 3 existed in America as early as 1849? I find them in late editions of Figgins's Specimen Books; but unfortunately their business has been sold to Messrs. R. H. Stevens & Co., and the new firm has not the means of finding out anything about origins of types.

I have not found these types in your specimen books.

P.S.—On second thoughts, I am sending you a couple of rotographs, one of the titlepage which contains the types I am writing about, and the other containing a drive-down * heading. The words "SOME RECENT LECTURES" are in the wide Latin type, and the words "The Origin of Life" are in the two-line long primer and brevier Monastic No. 2.

---

* A heading that is not printed at the very top of a page, but is driven down, as at the beginning of any important section of a book.

Mr. De Vinne, in acknowledging the receipt of this communication, said he was getting old and could not himself undertake the necessary research, but that he had handed Mr. Hart's letter and enclosures over to Mr. J. W. Phinney, the Manager of the American Typefounders' Company, Boston. Mr. Phinney kindly undertook a thorough investigation, which occupied some months, and it was not till Nov. 4, 1913, that Mr. Hart received the following reply :—

Mr. Theodore L. De Vinne has handed me your inquiry about the date of certain types used in the titlepage of a pamphlet entitled "Some Recent Lectures by George W. Sleeper; Boston, William Bense, Printer, 1849."

I have examined my specimen books of French, German, English, and American types, and am disappointed.

Our Librarian in Jersey City, where we have a Typographic Library and Museum, has examined the catalogues of four Libraries, and is unable to find the Sleeper Lectures pamphlet.

The type has been variously called "Norman," "Wide Latin," "Celtic No. 2," etc.

We do not find any specimens of "Norman" in French specimen-books earlier than 1879, in Berthier, a concern with a reputation of [producing] faces foreign to France. Even at this day no French specimen-book of a leading type-foundry shows "Norman." The earliest showing in England was by Figgins, 1873, one size, two-line Great Primer, Caps. and Small Caps., called "Wide Latin." The same book shows "Ornamented Latin," three sizes, with Floriated Caps. and Small Caps. similar to "Wide Latin," in bodies corresponding to our 6-, 8-, 10-, 12-, and 18-point sizes.

Figgins also shows the Floriated Caps. separately under the name "Latin Initials."

We have not been able to find the face in German books earlier than 1885.

MacKellar, Smiths, & Jordan's 'Typographic Advertiser' of July, 1870, shows the face in eight sizes as "Celtic No. 2." All other specimens in that issue are priced, indicating that the face was quite new at that date in that foundry.

The Matrix Book of the Johnson Foundry shows that the face was first added in 1870. The matrices are electrotyped. The specimen has no patent date, while all faces originating in that foundry were patented as they appeared.

Conner's 'Typographic Messenger' shows the face in 1873, as a supplement, printed on one side, suggesting that it was just added.

The Boston Type Foundry shows the face in five sizes of Caps. in 1874, and there is a note to the effect that "Small Caps. are in preparation."

Our collection has the Dickinson book of January 1876, in which "Norman" is shown in four sizes in Caps. and Small Caps. I believe the Dickinson commenced to manufacture the "Norman" in 1874.

Finally, in a letter written March 16, 1914, kindly giving me permission to quote the above statement, Mr. Phinney summed up the results of his enquiry in these words :— "It seems to me impossible that the titlepage could have been set at the date claimed for it, the style of type used in some of the lines of the titlepage being designed at a much later date, as we have recently established in the running-down we gave to the type face."

2. *Early Workmanship of W. Bense compared with that of the Booklet dated* 1849.

The rough appearance of the booklet was referred to in last year's address (p. 41), where Mr. Hart's opinion that it was the work of an amateur is quoted, and where it is compared unfavourably with the 1860 pamphlet.

Sir George Warner wrote, August 7, 1913, after seeing the booklet:—

> Apart from its really astonishing anticipation of modern theories and discoveries, the only thing at all suspicious about the pamphlet is that it looks almost too early and scrubby—in short, overdone, especially as it purports to have been printed at Boston, which was not a primitive Western town, but the literary centre of the States. Even if Bense was merely a printer in a small way of business, one would have expected something better, and it is a pity more of his work is not available for comparison.

It has, fortunately, been possible to supply this want; for Mrs. Endicott, a daughter of the late Mr. W. Bense, kindly sent to me on Nov. 4, 1913, two samples of her father's work set up within a few years of 1849. One is the 'Annual Report of the Selectmen of the Town of Canton' (1855); the other the 'Massapoag Journal and Canton Observer' (vol. i. No. 12, Oct. 9, 1852). Mr. Hart has kindly examined both of them, and tells me that the type referred to on p. 25 is not to be found in either, although the journal is rich in the variety of its founts. The workmanship of both is creditable, and bears no resemblance to the rough and untidy print of the booklet. The comparison affords strong evidence that the booklet was not printed by Bense.

Mrs. Endicott wrote in the accompanying letter:—

> My father thoroughly understood his business, and took pride and pleasure in it. He served an apprenticeship in the old-fashioned way with the well-established firm of Crocker and Brewster, continued with them afterward as a journeyman, and ever since my earliest recollection had his own printing-office in Boston, and I suppose was in business for himself at the time of his marriage. He must have had a proper outfit, too, for he made a specialty of dainty work, dance orders, bills of fare for some of the large city hotels, and other work of that nature.

Mr. Phinney, in his letter to Mr. Hart (p. 26), also stated that "William Bense was an excellent printer."

The copy of the 'Massapoag Journal' supplies evidence that there were business relationships between W. Bense and G. W. Sleeper; for it contains a rather extensive advertisement of the latter's "New England Tea Hong," at 130 Washington Street. Furthermore, the last advertisement in the paper, that of the printer, W. Bense, states that "orders may be left with E. & N. Bent, at the Depot, or at 130 Washington Street, Over New England Tea Hong, Boston."

Mr. J. F. Sleeper states that his father specially arranged for W. Bense to procure the paper and type from abroad, and would not permit the printer to damp the paper. No such stipulations appear in the very elaborate contract (see p. 32). The price, moreover, of 30 dollars for 500 copies, does not seem to leave much margin to pay for the special importation of paper and type.

3. *Evidence that G. W. Sleeper was described as a Printer between* 1866 *and* 1872.

In last year's Anniversary Address it is stated (p. 42), on the authority of Dr. H. Putnam, that G. W. Sleeper's name appears as a printer in the Boston Directories from 1866–1872. Mr. J. F. Sleeper, on the contrary, strongly asserted (*l. c.*, footnote) that the information was erroneous, and that his father went to New York in June 1866. I therefore asked Dr. Putnam if he would give me the precise addresses printed in the volumes. He kindly replied as follows :—

Office of the Librarian of Congress,
November 14, 1913.

The following entries occur in the Boston City Directories for the years 1866 to 1872:

1866. Sleeper, George, printer, boards 89 Hudson.
1867. Sleeper, George, printer, boards 12 Piedmont.
1868. Sleeper, George, printer, 120 Wash. b. 11 Suffolk-pl.
1869. Sleeper, George W., printer, 118 Wash. b. 29 Harvard.
1870. Sleeper, George W., printer, 118 Wash. b. 29 Harvard.
1871. Sleeper, George W., printer, 118 Wash. b. 29 Harvard.
1872. Sleeper, George W., printer, boards 10 Decatur.

It will be seen that two of the business addresses are very near that of the "New England Tea Hong," at 130 Washington Street.

Mr. J. F. Sleeper states emphatically that his father "never was a printer; any such statement in a Boston Directory for 1860, &c., is either a gross error or refers to some other person. Your proof said '1866–1872.' At that time father was in New York and New Jersey!" Mr. Sleeper also suggests many directions in which enquiries may be expected to yield confirmatory evidence. These I have not had time to make. Furthermore, any amount of evidence that G. W. Sleeper was resident in New York and New Jersey between 1866 and 1872 would not disprove his residence in Boston for portions of the same years. The proof that is required by Mr. J. F. Sleeper must deal with the Boston record. No other will suffice.

4. *The Contract with the Printer.*

The contract, of which a reduced facsimile appears opposite p. 32, is the most important document relating to the 1849 booklet that I have received. It supplies a crucial test. If it be genuine, the booklet receives sufficient confirmation, and will be accepted by the reasonable critic. If it be proved a forgery, the reasonable critic will conclude that the booklet is a forgery also.

Several eminent authorities in Oxford to whom I shewed the contract were quite satisfied with its appearance, and were prepared to accept the booklet on its authority. I do not think that any of them examined it very critically, for they appeared to be satisfied with the inherent improbability that it had been forged. Mr. Horace Hart remarked that the deed was unnecessarily pompous and elaborate for so small a sum as thirty dollars.

After receiving several favourable reports, I took the document to the Regius Professor of Modern History, Prof. C. H. Firth, who at once expressed his disbelief in it, telling me that he would not think of accepting historical data on such evidence. He did not consider that the brown stains looked genuine, and thought that the effect had been in part produced by rubbing.

Mr. J. F. Sleeper states that the lecture containing the word "Agnostic" (App. p. 15) and some other manuscripts, being crumpled and dirty, he damped them slightly with a nearly dry sponge, and then ironed them flat. Such treatment might have caused the appearance noticed by Professor Firth.

The document was then sent to Sir Frederick Kenyon, who kindly expressed his opinion as follows :—

British Museum,
London, W.C.
July 25, 1913.

I return the contract. I can't say I like the look of it. The brown stain does not look natural; it has more the appearance of the coffee-stain which one sees on faked documents than the genuine foxing. Otherwise there is not much to take hold of, though I should have expected more wear at the cracks of the folds. On the whole, though I should not like to pronounce decisively, this document rather increases my distrust than otherwise.

I have shown it to the present Keeper of MSS., who concurs; unfortunately we no longer have Warner available, whose experience is much greater than ours.

I next sent the contract to Sir George Warner, late Keeper of the Manuscripts of the British Museum, who was then at Llandrindod Wells. He kindly replied, Aug. 1, 1913 :—

I have carefully examined the document which you have sent me, but without further evidence cannot come to a positive conclusion respecting its genuineness. So far as its general appearance goes, it certainly *might* date

from 1849, but I am by no means convinced that it really does so, for there is much in it that is decidedly suspicious. The discoloration (which in forgeries is so often a weak point) does not look quite natural, and I see no signs that it is caused by the use of pounce instead of blotting-paper*. But, apart from this, there seems to be an occasional tendency to lapse into what may be the writer's own customary hand as distinct from an artificial one. Take, for instance, the "ð" at the end of "aforsaid." This was originally written as "ð," but has been painfully altered to "*d*" so as to conform to the usage elsewhere; and the same is apparently the case with the "*d*" at the end of "counted." In my experience there is no letter, especially when at the end of a word, in which a forger is more often taken off his guard; and throughout the document the *d*-form seems to be made with more or less of an effort, as if the writer was not accustomed to it. I am also inclined to think that the attestation was written by the same hand as the body of the document. The mode of connecting the letter *s* with *e* or another letter following is somewhat peculiar and characteristic, and it is exactly the same in "presence" and "Joseph" as it is in "presents" (l. 1) and elsewhere†.

A few weeks after the receipt of the above letter, Mr. J. F. Sleeper kindly sent several sheets of his father's manuscript, written within a few years of 1849. These were submitted, together with a collotype of the contract, to Sir George Warner, who wrote, Aug. 28, 1913, as follows:—

The more I see of the contract the less I am disposed to believe in its authenticity, for the hand varies too much in so short a document written all at one time to be the writer's own natural hand, and has all the signs of an attempt to disguise it. The paper, so far as I remember it, seems to be the same as that used for most of the documents now sent, but of course it may have been a blank page torn off for the purpose of the forgery, if it is one, as I more and more strongly suspect is the case. I certainly would not commit myself to a belief in the pamphlet until more copies with authenticated dates of acquisition are forthcoming.

It was obvious, after learning Sir George Warner's opinion, that every attempt should be made to obtain signatures for comparison with the contract, as well as other possible confirmation. On Aug. 9 I wrote to the Librarian of the Boston Public Library, asking if he could kindly give me the names and addresses of

* Mr. J. F. Sleeper sent me samples of his father's white and also of his dark blotting-sand, but they bore no resemblance to the fine white powder which was thickly encrusted on some of the ink.

† Sir George also wrote on the same subject, Aug. 7, 1913:—

In the contract it is not the similarity of the signature of Bense to the body of the document which struck me as a little suspicious, for I think there can be no doubt that, as would be natural, they were both written by the same hand. The question is, whether the signature of the witness was not also written by the same hand, though the intention was to disguise it.

These of course are only grounds for suspicion, and not proofs, but they make me very anxious to see specimens of both Sleeper's and Bense's handwriting, if it is possible to obtain them. The problem presented by the pamphlet is an extremely interesting one, and it is well to sift the matter thoroughly. . . . . The use of the term "Agnostic" staggers me, and I confess I am sceptical and disposed to ask for more proof than this doubtful contract that the pamphlet is anything more than a fraud prompted by an old man's vanity and desire to score off his detractors.

W. Bense's children. The Assistant-Librarian, Mr. Otto Fleischner, kindly replied, Aug. 22, sending the addresses of two daughters and a son. I wrote at once to Mrs. Frederick Endicott, of 1032 Washington Street, Canton, Massachusetts, who replied most kindly on Sept. 21st, and has since continued, in consultation with other members of her family, to assist the investigation. Mrs. Endicott's letter showed that no evidence was to be expected from the business books and papers that must have been kept at her father's printing establishment, for

in the panic and confusion of the great Boston fire in 1872, the chief of the fire department ordered the blowing up of numerous buildings, among them the one at 35 Congress St., where his office was. The fire did not reach the spot, but the great building was totally wrecked, and father's finely equipped office, presses, and everything, went down in the ruins. So of course everything, books, papers, and all were gone, and he had to make an entirely new beginning.

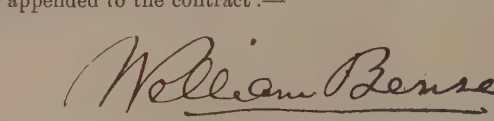
Mrs. Endicott had never heard of Mr. Joseph Crafts, and other attempts to trace the witness to the contract have also ended in failure. Enclosed in Mrs. Endicott's letter was the signature of William Bense, reproduced on a slightly smaller scale below. The signature was the receipt for a bill dated Jan. 8, 1890, from the office at 35 Congress Street, Monks Building:—

The signature was obviously closely similar to that upon the contract (opposite p. 32). I sent it to Sir George Warner, together with a collotype reproduction of the contract, and wrote to Mrs. Endicott begging for more signatures, especially of an earlier date, and one as near as possible to 1849. Sir George replied, Oct. 6, 1913, as follows:—

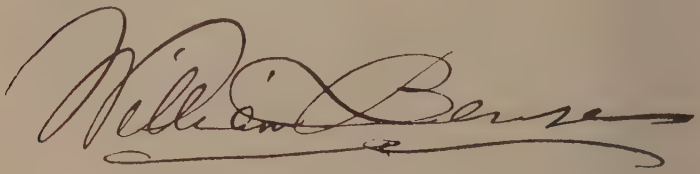
The signature you have sent me certainly does support the genuineness of the Sleeper-Bense contract of 1849. At the same time I am not quite satisfied, for it seems to me very remarkable that after so long an interval as 40 years the signature should be so precisely identical; and it is almost easier to believe that the early one is a forgery from a considerably later example. Is there no signature of Bense more nearly of the same date as the contract?—and other writing as well, for it is the text of the contract which looks so suspiciously irregular and artificial. The signature, I think, would not be a difficult one to forge.

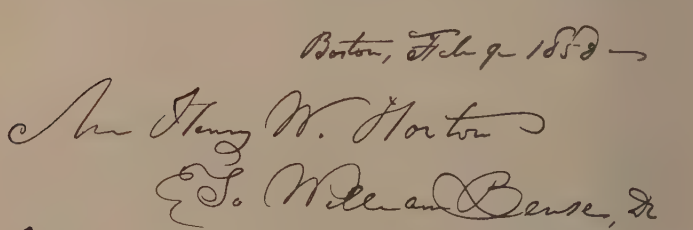
The hoped-for signatures, three in number, were found by Mrs. Endicott and her sister, and sent Nov. 4, 1913. One of them, reproduced in a slightly smaller size below, is of late date, Sept. 5, 1882. It is a receipt from the same office as the

signature last-mentioned, and, like it, obviously resembles the one appended to the contract:—



The two other signatures are of great importance, being much earlier—the first, of Dec. 17, 1856, not eight years, the second not quite nine years, from the date of the contract; yet both are very different from the signature on the latter. The first, the receipt on a bill, is shewn slightly reduced; the second, on an unreceipted bill, is of about half the original size, and includes with the signature the upper third of the document, so that the ordinary handwriting, as well as the signature, may be compared with the contract:—





Sir George Warner wrote, Nov. 17, after receiving the four signatures and the collotype of the contract:—

I have received your five documents and have compared them. The two dated in 1856 and 1858 supply the evidence which has hitherto been lacking, and I am now convinced that the contract purporting to be dated 18 May, 1849, is nothing more than a forgery, as I have suspected all along. Both the text and signature were evidently written by the same person; but the hand of the text has no resemblance to that of the items in the bill of 1858, nor

Know all men by these presents that I William Benson of Boston, County of Suffolk for & in consideration of thirty dollars to me in hand paid this day by George W. Sleeper of Boston aforesaid, the receipt whereof is hereby acknowledged, do by these presents agree to print, bind and deliver to the said Sleeper five hundred copies of a pamphlet — title: "Some Recent Decisions," — paper included & pages not to exceed forty, cover & title-page also counted.

In witness whereof I have hereunto set my hand & seal this eighteenth day of May, A. D. Eighteen hundred & forty-nine.

In presence of
Joseph W. Bratt

William Benson

REDUCED COLLOTYPE FACSIMILE

Size of original sheet $9\frac{9}{10}$ *in.* × $8\frac{1}{10}$ *in.*

could the latter, I think, have been developed from it, while the signature appears to have been copied from a genuine signature of Bense later, and probably much later, than 1858, after he had ceased to make the exaggerated flourish at the end of B.

I am going up to the British Museum on Wednesday and will show the documents, if I may, to Mr. Gilson and get his opinion.

Two days later, Nov. 19, Sir George wrote :—

I return the documents, which I have shown to Mr. Gilson, Keeper of MSS. at the British Museum. He agrees with me that the contract is spurious.

5. *Advertisements for the Booklet dated* 1849.

In the course of a letter written Aug. 7, 1913, Sir George Warner had made the following suggestions :—

How would it be to advertise for further copies of the pamphlet; for, if 500 were printed, others almost certainly exist somewhere, and the date of acquisition in some cases might be recorded or be otherwise ascertainable?

Such attempts have now been made.

Dr. Putnam very kindly advertised for a copy of the 1849 booklet, but without success, as explained in his letter of Feb. 13, 1914 :—

We advertised for Sleeper's "Shall we have Common Sense?" in the 'Bookseller, Newsdealer and Stationer,' and also in the 'Publishers' Weekly,' on November 10, 1913, but no reply of any sort has been received to the advertisements. Our failure to find by this means a copy of the edition of 500 copies supposed to have been issued 65 years ago would not appear *per se* to argue such an issue as improbable.

With regret at our inability to aid you further in this matter.

Mrs. Endicott also kindly advertised in November, 1913, for information concerning the pamphlet in the 'Boston Transcript,' and has recently drawn my attention to another advertisement to the same effect which appeared at the beginning of last March in the same paper. No reply was evoked by either of these.

6. *The Entries in G. W. Sleeper's Diary for* 1849.

The diary, kindly sent to me by Mr. J. F. Sleeper, was a little bound volume for the year 1849, so arranged that each of its pages held spaces for the entries on three days. The entries had been struck out, sometimes with pen, sometimes pencil, but were still legible. Under the date Thursday, April 26, was the entry "Copyright ·50," and under Friday, May 18, "Bense Pamphlets $30.00." If other details of the expenditure of a prosperous business man, as Sleeper was then, had been entered with equal care, the diary would have been far more crowded than as a matter of fact it was. The page including April 26 held no entry except the "Copyright," the opposite page held two entries; the page including May 18 held three entries in addition to the "Pamphlets," the opposite page two entries.

I was anxious to know if the fifty cents conformed to the law at the time, and also whether there was any entry in the record book for April 26, 1849, that might have escaped notice. I therefore wrote to Dr. Putnam, who very kindly sent me the following result of a renewed search made by the Register of Copyrights :—

From the REGISTER OF COPYRIGHTS,
Sept. 12, 1913.

To the LIBRARIAN OF CONGRESS,

Referring to the inquiry from Prof. E. B. POULTON.

Professor Poulton asks that the Copyright Office record book for the year 1849 be searched under the specific date April 26, for possible entry of Mr. Sleeper's book. We have done so, but find no registrations at all on April 26 of that year. As stated in my previous "Memorandum," we had already examined the record book page by page for the year 1849 without discovering any entry in regard to Mr. Sleeper's book.

He also asks, in view of a notation in Mr. Sleeper's Diary, what the legal copyright fee was at that date, and whether the copyright could have been registered before the book was published.

The answer is that any registration in 1849 must have taken place under the provisions of the Act of February 3, 1831, which provided in sec. 4 as follows (in part):—

> "That no person shall be entitled to the benefit of this Act, unless he shall, *before publication*, deposit a printed copy of the *title* of such book, or books, map, chart, musical composition, print, cut, or engraving, in the clerk's office of the district court of the district wherein the author or proprietor shall reside, and the clerk of such court is hereby directed and required to record the same thereof forthwith, in a book to be kept for that purpose . . . . For which record, the clerk shall be entitled to receive, from the person claiming such right as aforesaid, *fifty cents* . . . . And the author or proprietor of any such book, map, chart, musical composition, print, cut, or engraving, shall, within three months from the *publication* of said *book*, map, chart, musical composition, print, cut, or engraving, deliver or cause to be delivered a copy of the same to the clerk of said district. . . ."

Respectfully,

THORVALD SOLBERG,
Register of Copyrights.

It is therefore clear that the date and fee of the copyright are entered in accordance with the law existing at the time; although, in the absence of confirmation, they are valueless as evidence of the publication in 1849. Sir George Warner, in fact, considered that the diary does not support the genuineness of the pamphlet, for he wrote, Sept. 6, 1913 :—

I am not satisfied with the entry in the Diary, 18 May, and instead of removing my suspicions it merely increases them. The ink in which it is written looks as if it had been doctored to give it the appearance of age, and neither this entry nor the others in which the same ink is used have quite the character of G. W. Sleeper's hand—they seem imitative rather than natural, *e. g.* compare "Burrill," 20 May, with "Burrill," 15 March, and the entry on 22 May with the other "Nash" entries. The object of more forged entries than one would of course be to make any peculiarity in the appearance of the entry less marked. On the other hand, assuming all the rest of the entries to have been Sleeper's, his hand certainly does vary to a certain extent, as everyone's does when writing at different times and under different conditions. So

far therefore the evidence is inconclusive, and I am very glad to hear you are in communication with Bense's daughters. If his ledgers or business accounts are in their possession, they ought to settle the matter *; but it will be something, with regard to the contract, to get specimens of his signature and handwriting †. I am inclined to think that the 18 May entry and the contract were written by the same person, so far as I can remember the latter.

7. *Manuscript exhibiting Appearance of Artificial Treatment with a Brown Stain.*

Mr. J. F. Sleeper kindly sent to me, Aug. 10, 1913, a number of short manuscripts, probably the original rough drafts of a series of articles for the press entitled "Thoughts of an Atheist." Some of the pages were written on the backs of sheets dated 1851 and 1852, and accompanying them was a manuscript letter, protesting against slavery, signed "A Southerner," and dated Aug. 14, 1852. This, like the "Thoughts," was evidently intended for the press, and probably published. There seems little doubt that all were written within a few years of 1849, yet the contrast between them and the pamphlet is immense. The "Thoughts," which convey no suggestion of originality or power, include discourses on the following subjects:—(No. 7) On laws and their enforcement and on the atheist being compelled to give evidence on oath—although meaningless to him; (No. 8) On the atheist being as good as any other man; (No. 12) On the brain of animals and man, and on mind as the product of brain and therefore of matter; (No. 13) On animal sagacity and instinct; an unnumbered paper on the inconsistencies of the attributes of the Deity.

No. 9 was of interest in a very different way: for it had obviously been treated with a brown stain, apparently laid on by a brush or rubbed on by a broad surface, perhaps the end of a finger or a painter's stump.

Mr. J. F. Sleeper suggests that a fluid may have been spilt over the manuscript, or mildew formed in the creases, and that after some chemical had been then brushed over the surface in order to remove the effect, the iron in the ink had in time caused the stain. He has not seen such an appearance on any of the other papers.

This curious-looking document was submitted to Sir George Warner, who wrote Aug. 28, 1913:—"No. 9 has a very queer appearance. Is the stain tobacco-juice or what? It is obviously artificial and intentional, and not the natural discoloration of time like that on No. 8; but this again, I think, is not the same as that which is on the contract."

8. *The Paper of the Booklet dated* 1849.

The present section does not contribute evidence for or against the authenticity of the work; but it is desirable to render the

* See p. 31. † See p. 32.

enquiry as thorough as possible, and to throw further light on a subject already investigated up to a certain point.

Mr. Lewis Evans and Mr. Johns expressed the opinion, at the time of last year's address (p. 40), that the paper is probably a Dutch hand-made.

The late Mr. De Vinne also informed me, Aug. 25, 1913, that "the hand-made paper sold in the United States about 1850 was largely of Dutch manufacture."

It is now possible to come to a definite conclusion concerning the watermark, which appears on pp. 19–20 of the booklet, and concerning the letters I V to be made out from pp. 9–10 and 25–26.

Last summer, Mr. Horace Hart, continuing the skilled help which he has always given me in this enquiry, sent a photograph of the watermark to Mr. C. H. Balston of W. and R. Balston, Ltd., Maidstone, proprietors of the Whatman paper. Mr. Balston recognised the device as that distinctive of his firm for paper of Royal size (19 in. × 24 in.), namely, a shield with three diagonal cross-wires. He furthermore stated that the form of the device was of an old type, shewn, by a fine series of samples kindly submitted by him, to have been altered by 1831, when a broader bolder pattern was substituted, and has been continued unchanged to the present day. Unfortunately the photograph sent to Mr. Balston did not include the lowest part of the device, which in the Whatman watermark terminates with a W, but in the pamphlet with a simple point, making a V. I wrote explaining this to Mr. Balston, who replied, Nov. 11, 1913:—

> If, as you say, the monogram in the watermark finishes off as a V, and not as a W, then I agree with you that the device is probably a pirated one. I noticed in your photograph the centre of the W did not appear, but as it was on the extreme edge of the print, I thought possibly that its non-appearance might be due to a fault in the printing, or that it appeared lower down, so I did not attach much importance to it. Another point which struck me was that the upright lines were not the regulation distance apart, namely $1\frac{1}{16}''$, which is invariably the case in all the samples I have of this size from 1781 up to the present day. In the photograph showing the monogram they are 1″ apart. This might be due to shrinkage of your printing paper after washing the print, so I attached no importance to it. You could, however, verify this point by measuring the original. I have made a fairly extensive search through all the old papers and have been unable to find any watermarks with the characters I V, as referred to in your letter of July 14.

The letters I V are clearly to be explained as a fraudulent rendering of J. W. (for James Whatman), which appear on the earliest Whatman sheets, viz. 1781 and 1784 in the specimens sent by Mr. Balston. It is probable that the imitated paper was of very early date; for in 1808 and 1810 J. W. is replaced by J. Whatman, while W. Balston is added, although omitted from later years, viz. 1831 and 1834.

The upright lines of the pamphlet are not spaced at regular distances, some of them being 1 in. apart, others as much as $1\frac{1}{8}$ in.

Considering the evidence as a whole, there can be little doubt

that the paper of the pamphlet is of Dutch manufacture and that it bears a forged Whatman watermark of early date in the history of the firm. Having undertaken this enquiry I have been anxious to pursue it to the end in every detail, although fully aware that the identification of paper and watermark throws little light on the date of the pamphlet, for in the words of Mr. Lewis Evans:—

> Should anyone in America, say in the seventies, have contemplated a literary forgery purporting to belong to the forties, he might easily have got enough suitable paper for the purpose of printing a few copies, either from some old blank books or conceivably from some little lot in stock in a printer's warehouse . . . . Aug. 18, 1913.

9. *A Manuscript Lecture entitled "Life and the Cosmosite" as Evidence for the Booklet dated* 1849.

Mr. J. F. Sleeper has kindly permitted me to see the following letter addressed to his father:—

Boston, Nov. 5/50.

My Dear Sir,

You are cordially invited to deliver your Lecture—Life and the Cosmosite at the meeting of the Society * next Monday.

Ever &c.,

James W. Stone †.

Mr. Sleeper.

Accompanying the letter was a manuscript of rather closely written pages ($8\frac{3}{8} \times 6\frac{3}{8}$ in.), bearing the title "Life and the Cosmosite," and purporting to be the lecture of Nov. 11, 1850, delivered in response to Dr. Stone's invitation. The lecturer

* The Massachusetts Medical Society is the only one which Mr. H. G. Wadlin, Librarian of the Public Library of the City of Boston, can suggest. He kindly wrote, Feb. 12, 1914:—"A search of the Boston newspapers of Monday, November 11, 1850, and previous and subsequent dates, revealed no notice of the lecture by G. W. Sleeper.

"No mention of the lecture before the Massachusetts Medical Society is found in 'An Index of Medical Communications, Library of Practical Medicine, and Publications of the Massachusetts Medical Society, 1790–1901. Prepared by the Recording Secretary. Boston, Clapp, 1903, 40 pp., 8°.'"

G. W. Sleeper's lecture would hardly fall into the category of "Medical Communications," so that the absence of a record is probably unimportant.

† The following information has been very kindly sent to me, Feb. 12, 1914, by Mr. Horace G. Wadlin:—

The 1850 Boston Directory gives the address of Dr. J. W. Stone as 6 Bowdoin Square, h. do. "The Harvard Medical School, a History &c. by Thomas F. Harrington," Vol. 3, p. 480, No. 794, has the following record of Dr. Stone:—

"James Winchell Stone, A.B. 1843; A. M.; b. Oct. 26, 1824, Boston; d. Aug. 20, 1863, Dorchester. Practiced Boston. Collector Internal Revenue, 3d. Massachusetts District. M. M. S. S." From 1857–1862 Dr. Stone's residency was Cottage Street, Dorchester.

"Harvard College, Class of 1843, Memorabilia, 1883, Prepared by Wm. A. Richardson," has the following entry relating to Dr. Stone:—

"James W. Stone, Boston (Roxbury, Mass.). Descendants, Ella G. Stone Florence, Italy; Frances Tyler Stone, Florence, Italy."

speaks of being "about to address you this evening at the liberal invitation of Dr. Stone . . . ." He continues, after a brief introduction, "In 1847—only three years ago in this very city—some of you will doubtless remember I delivered several Lectures, one on the Origin of Life and the Advance of Civilization, ..." He then goes on to speak of their hostile reception, concluding this part of his lecture in the following words:—"These good Christians, ... were also zealous to show their pious faith, by publicly burning my 'blasphemous pamphlet,' that is, the few that they could lay hands upon . . . . All this tempest rose because a man who had thought and experimented and reasoned and reflected tried to place before his fellow-men the results he had theoretically and experimentally arrived at" . . . . "It is this theme (with your kind permission) which I intend to review to-night, and in doing so I hope to reveal my thoughts in a somewhat clearer manner—." The lecture proper then opens with a paragraph beginning—"As I then was at pains to try to prove" and continuing with the substance—often word for word the same—of the second and third paragraphs on p. 3 (p. 3 of the present Appendix) of the 1849 pamphlet. The last three lines "may be greatly modified . . . . its kind" on p. 3 appear word for word on a later page of the lecture. The passage referring to the triad of Lemur, Monkey, and Man (Appendix p. 5) also appears in nearly the same words. The two sequences of stages (Appendix p. 8) are also given—fused together and almost complete. The lecture concludes, as does the 1849 pamphlet, in argument directed against the existence of a Personal Deity. As evidence there is brought forward the "fact . . . . . that the air, &c., is filled with myriads of deadly germs." Apart from the above, the lecture is almost entirely devoted to the contention that the germ of every animal and plant has always existed and will continue to exist for all time, that it may lie dormant for long periods until called into activity by favourable circumstances which determine the particular form its reincarnation will assume. All germs are apparently to be traced back to an electrical primal germ containing "within it all the essentials of all Life that it is possible to create—Cosmosite . . . . or 'germ-world-inhabitant.'"

It will be realized from the above account that the lecture itself is of no great interest or importance, except as supplying evidence that the booklet dated 1849 is genuine. The validity of this evidence again rests upon the authenticity of Dr. Stone's invitation. Accompanying the latter were three other documents signed by J. W. Stone. One of these dated Aug. 11, 1856, introduced "my friend George W. Sleeper, Esq." to the Hon. R. G. Hazard. A second of the same date, written to G. W. Sleeper, suggested that the same introduction might be shown to others, who are named. The object in view was probably political, viz., "doing something for the good cause of Frémont and Dayton."

A third document, dated Boston, May 23, 1854, certifies that Dr. Stone had known "George W. Sleeper for some years as an active, determined, paying, and working member of the Suffolk Free Democratic Ward & Co. Convention, of which he was one year the Secretary."

There was something in the handwriting of the invitation, when compared with that of the three other documents, which roused my suspicions. I sent all four to Sir George Warner and asked for his opinion. He kindly replied as follows, April 18, 1914:—

I return the four additional letters which you have sent me. I am unhesitatingly of opinion that the one dated 5 Nov., 50, is spurious. It is almost enough to place the four letters side by side, when the difference of its handwriting from that of the others is at once apparent. Instead of being easy and natural, it is laboured, halting, and plainly imitative. But there is other damning evidence. The paper (*a*) is the same make as that of the letter dated 23 May, 54 (*b*), and is, I have no doubt, the originally blank second leaf of the same sheet. You will see that the left edge of *b* is slightly rough, showing that it has been cut with a paper-knife. If you lay *a* upon *b*, making the right edges exactly even, you will find that the width is not quite the same, and that the left edge of *a* has been trimmed (so as to avoid a suspicious rough edge), but not quite straightly. It has also been carefully torn, evidently with design, so as not to show the same holes as are in *b*, and also for the purpose of destroying the evidence of the date in the endorsement. Nevertheless, by laying the one leaf carefully over the other, as I have said, the top hole and the next will be seen to fit in exactly. This would hardly be the case unless *a* and *b* were the two leaves of the same sheet, any more than the original folds would exactly coincide, as in fact they do.

The folds spoken of by Sir George Warner traverse the leaves in both directions—three across and two longitudinally—so that the case against coincidence is much stronger than it would be with the two leaves of most letters. The four holes are perforations by which the letter had been doubtless attached to others in pamphlet-like form, and a part of the circumference of the third hole as well as that of the first and second is still visible, in spite of the torn edge. The only one that has been entirely removed is the fourth and lowest. Examination with a lens shows that the portions of the three upper holes resemble the corresponding parts of the entire holes in their darkened colour as well as their precise form. The endorsement has not been wholly torn away. "Dr." and "18," the first two figures of the year, were no doubt intentionally left. The blank sheet was probably damped and ironed in order to alter its appearance as far as possible.

Sir George Warner's convincing proofs that the invitation was forged are, of course, sufficient evidence that the manuscript lecture, which opens with a reference to the invitation, is also fraudulent.

10. *The Use of the Word "Agnostic."*

Mr. J. F. Sleeper wrote May 10, 1913, stating that the word Agnostic was invented by his father in 1846 and that he used it in several lectures. The manuscript of the lecture in which the word was introduced was the first of all documentary evidence

submitted to me. The lecture is entitled "*On Our right to express our Ideas and the value to Mankind of that expression.*"

Speaking of "men who dared to *think* differently from their less-educated or less-original fellows," and who, although persecuted in their own age, were honoured in later generations, the manuscript continues:—

> These men I shall designate as "Agnostics"—as differing from the mere Atheist, under this term I comprehend much: I mean it to signify a man of advanced ideas who confesses he has *not* attained perfect worldly wisdom, who admits he does not know all about "God" and "a future state," who is an honest asserter of ignorance on all matters not proven to him by actual Scientific demonstration, and to whose reflective mind the mere verdict of a majority carries little weight. Which, as you will see, is precisely the reverse of the word "*Gnosis*" and of the professions of the old Gnostics (as described by the Rev. Taylor in his Diegesis on page 37)—in several respects—and is also in defiance of those bigoted, all-knowing, persecuting and pitiless imparters of divine revelation whom I would call Torarians from the Hebrew word *Torah**.

The manuscript is signed with the initials "G. W. S.," and is undated. Mr. J. F. Sleeper informs me that his father said the lecture was delivered during the latter part of 1846. The date can be inferred by means of a quotation from the "poet Mackay," who is said to have "just composed the lines—

> " . . . . . We wonder long
> That hate had power to lead our fathers wrong
> Or that false glory lured their hearts astray
> And made it virtuous and sublime to slay."

Mr. J. F. Sleeper tells me that these lines form part of a work entitled 'Railways' dated 1846.

The circumstances under which Huxley introduced the word "Agnostic" in 1869 were quoted in last year's Address (pp. 44–5). Although no comparison is possible between the literary form of the two accounts, the line of thought is precisely the same, and adds another to the long series of improbable coincidences involved by the hypothesis that the booklet is genuine. But the facts proved in sections 2, 4, and 9 show that it is not genuine, and we are driven to conclude that the manuscript is a late fabrication, and that the manner in which the date is revealed without being stated, as well as the addition of "Torarian" are parts of an elaborate scheme intended to throw the critic off his guard. It was probably foreseen that there would be a tendency to argue thus: "The date is so important that a forger would certainly have added it: therefore, as it has not been added, the document is genuine."

11. *The* 1860 *Pamphlet is inconsistent with the Booklet dated* 1849.

It was pointed out on p. 43 of last year's Address that the comparison between the two pamphlets was unfavourable to the

* Signifying inspired teaching.

authenticity of the one with the earlier date. A further serious inconsistency was pointed out to me by Mr. Howard M. Chapin, Librarian of the Rhode Island Historical Society, Providence, and was afterwards independently observed by my friend Professor Lotsy, of Haarlem. The Introduction of the later pamphlet opens with the words

> This lecture is founded upon a theory, entirely original upon this side of the Atlantic, and which first developed itself in my mind about the year 1853 — at which time I published a small article in a Boston paper, from which nucleus was produced and delivered the greater part of this discourse, in the year 1856, before the Hopkins Literary Association of North Providence—of which I was then President—in which capacity, I became acquainted with F. W. Miner, who, to reciprocate a like act, invited me to deliver it before the South Providence Lyceum, where it was also well received.
>
> The next delivery of it occurred from an invitation to present it as the first, or opening lecture of the "Member's Course of Franklin Lyceum Lectures," which was done, and it stands so recorded—notwithstanding every means have been used to efface it.

The Introduction then goes on to speak of the attempts made to deliver the lecture on Feb. 8 and again on Feb. 24, 1860.

I have quoted the above sentences in order to shew that the author was extremely careful to give the history of the development and the exposition of his thoughts on education in relation to civilization. And yet these very thoughts are set forth in considerable detail on pages 12–18 (also 12–18 of the Appendix to the present Address) of the 1849 booklet, stated in the Introduction to contain lectures delivered in 1847, and to be founded on ideas which entered the author's mind when he was 17, viz., in the year 1843. We cannot doubt that if the pamphlet dated 1849 had been in existence in 1860, the author would have referred to it in his Introduction.

Mr. J. F. Sleeper maintains, on the contrary, that there is a real difference between the two pamphlets, which accounts for the want of any reference from the later to the earlier. He states that in 1847 his father looked upon Education as the offspring of Civilization, but in the next decade regarded Civilization as the offspring of Education. I am sure, however, that no one can read pages 12–18 of the accompanying Appendix without recognising the fact that Education is given the primary place, just as in the 1860 pamphlet, and that "the advance of civilization" spoken of in the title (Appendix, p. 1) was a progress which the author maintained in that very lecture to have been brought about by "Mother Education" (Appendix, p. 17).

12. *The Booklet contains Ideas implying Knowledge much later than* 1849.

My friend Professor Van Bemmelen, of Gröningen, has pointed out to me that the ideas on the germinal origin and germinal transmission of characters set forth on pp. 10, 11, and 14 (also

pp. 10, 11, 14 of the Appendix) of the 1849-booklet, and quoted in last year's Address (p. 31), imply a standpoint of knowledge and hypothesis that is much later than 1849. The passages referred to pre-suppose the equality in heredity of the germ-cells of the two sexes. Yet this conclusion is only implied and not announced as the important new thought it would have been in 1849. The same criticism applies to the work as a whole, which, apart from the discoveries announced in it, seems to belong to the intellectual atmosphere of a far later date.

My friend Professor August Weismann tells me that the conclusion that the male and female germ-cells are essentially similar, and play the same part in heredity, was first published in his memoir "Beiträge zur Naturgeschichte der Daphnoiden," Leipzig, 1876–79, p. 323.

13. *The "1849" Booklet probably of very recent Date.*

In May 1913 I had no reason to doubt the history of the copy given on pp. 26–27 of last year's Address, and considered on p. 43 to be the strongest evidence in favour of authenticity. The facts that have now been proved make it impossible to accept this history without further confirmation, and as yet such confirmation is wanting.

The attempt was made to trace the book-store at Cleveland or Cincinnati where Mr. Miller stated that he had bought the booklet (pp. 26–27). A letter directed to "Mr. W. Davie, Book Store, Cleveland," was returned with the stamp "Not in Directory No. 8." A similar letter directed to Cincinnati was not returned, but brought no reply, and a second letter to the same address was equally unsuccessful. My friend Dr. Joseph L. Hancock, of Chicago, has been unable to find evidence in the Directories that anyone named W. Davie is now keeping a book-store in either city.

The attempt to obtain confirmation has therefore failed, and we cannot accept the history of the copy sent to Dr. Wallace. If the booklet is a forgery, as there is the strongest evidence to prove, it is only reasonable to suppose that it was printed after *all* the discoveries announced in it had become known, and perhaps after the death of G. W. Sleeper in 1903. This suggestion was made by Sir George Warner in a letter dated Aug. 17, 1913:—

> The possibility of a fraudulent concoction after G. W. Sleeper's death had occurred to me. If, however, Miller's statement that he purchased his copy of the pamphlet in 1892 is correct, this cannot be, and the fact that the pamphlet is mentioned * in the obituary notice of 1903 is also against it . . . . But, on the assumption that the date 1849 is genuine, the wonder to me is that the author apparently does not refer to the theories advocated in this pamphlet anywhere else among his later publications, and that no claim to anticipation has been made on his behalf until now, although he lived to so

* This is a mistake (see p. 43).

late as 1903, and his son shares his views and seems fully alive to his pre-eminent merits . . . . . I am still rather sceptical, and look forward with interest to the arrival of the Diary, which ought to settle the matter as you say . . . . .

As a matter of fact, the pamphlet dated 1849 is *not* directly mentioned in the obituary notice of 1903.

When the account of G. W. Sleeper in the 'The Truth Seeker' for October, 1903, is carefully compared with the further references to him in the obituary notice of his wife in the same journal, for January 13, 1912, certain differences are apparent.

The earlier notice speaks of his "little book, entitled 'Education and its Offspring—Civilization,'" of "various ideas put forth in his numerous essays and speeches," and states "he was the first to advance the theory that germs cause most diseases, and further maintained that life was due to other organisms diffused throughout the atmosphere, requiring only favorable conditions for its development. Numerous novel thoughts concerning civilization likewise emanated from his mind, ..."

There is no reference to the 1849 booklet, and the words I have quoted give a very imperfect idea of its power and scope. And yet a sentence in the notice, quoted as one of G. W. Sleeper's "oft-repeated utterances," appears almost word for word * in the pamphlet, viz., the sentence beginning "Of what lies beyond the grave" and ending "need not fear to die" near the top of p. 17 (also p. 17 of the Appendix to this Address).

The references in the obituary notice of 1912 are very different. "The Origin of Life" and "The Dangers of the Unseen" are spoken of as lectures published in 1849, while the main subjects of both are set forth in brief but adequate summaries.

The above comparison strongly suggests that the 1849 booklet was printed after G. W. Sleeper's death. The printer's signature to the contract taken alone is probable evidence for thirty or forty years after 1849. The special mention of the house-fly as a carrier of disease is evidence for a much wider interval.

I have carefully read more than once everything in print and everything in manuscript by G. W. Sleeper that has been sent to me by his son, and I fail to see any evidence of that ability which would certainly be required to produce the booklet if it were written to-day. The evidence of ability is probably chiefly displayed in fitting the accounts of modern discovery into the framework of an older phraseology. The conclusions of modern science may well have been derived, as Sir George Warner suggests, from a *résumé* of recent discoveries in some scientific publication or magazine.

On the other hand, the fact that G. W. Sleeper is described

* The only verbal differences are the substitution of "can" in the notice for "may" in the booklet, and of "all his fellow-men" in the former for "all" in the latter. There are also slight differences in punctuation, capital letters, &c.

as a printer (p. 28), and that some of his manuscript shows the signs of artificial treatment (p. 35) are in favour of the conclusion that the fraud was perpetrated by him.

14. *The probable Motive of the Forgery.*

The consideration of motive seems to be almost a waste of time, but so many friends have raised the question "Why should anyone have done such a thing?" that I think it may be interesting to attempt a brief discussion. The most probable explanation is, I think, as follows:—

G. W. Sleeper was a man who loved to lecture and to take every opportunity of publicly expressing his views. He was probably far more wounded by ridicule and neglect than by any violent attack. The following reminiscence by an old resident of Providence was kindly sent to me by Dr. Putnam:—

> I knew George W. Sleeper by sight, but was not acquainted with him. He kept a tea store as stated. He advertised a lecture about the year 1860, the subject being 'Education and its Offspring, Civilization.' He may have put this in pamphlet form, but I do not remember of seeing it. There was a tendency to ridicule his literary aspirations and he was hardly taken seriously. Excepting in that early time I do not remember of ever hearing of him, and I do not know that he remained in Providence.

Sleeper's intense belief in himself would only have been strengthened by opposition, and still more by his failure to command serious attention. He probably quite honestly believed that the vague ideas which occurred to him were of momentous importance in the history of science, and that when he had put the results of his reading in his own words he was announcing original discoveries. Then, as the years went by, such ideas as natural selection, the origin of man, the continuity of the germ-plasm, the germ-theory of disease, resistance, the mosquito and the house-fly as carriers, sprang into existence and instantly became centres of intense interest. In each of these successive discoveries and many others, Sleeper's self-centred egotism would recognize the natural outcome of thoughts received so very differently or not received at all when he had spoken them. One thus self-deceived as to the importance of his own ideas would certainly honestly believe that he had been, and was still, the victim of bitter injustice, and he might defend the falsification of evidence on the ground that only in this way could justice be done not only to himself, but to the history of thought. He would argue "All these vaunted discoveries are the simple and easy development of original thoughts which I announced half a century ago, and it is only fair and right that they should bear the date of their conception as well as that of their birth."

The above attempt to analyse the psychological situation assumes that the forgeries were committed by G. W. Sleeper himself; but the same explanation may be offered if we suppose that they were committed by another who knew his feelings and shared his delusion that he was the victim of injustice.

Upon the conclusion of the Presidential Address, Dr. A. Smith Woodward, F.R.S., moved the following Resolution:—

"That the President be thanked for his excellent Address, and that he be requested to allow it to be printed and circulated amongst the Fellows," which, being seconded by Mr. Miller Christy, was carried by acclamation.

The President then addressing Prof. E. A. Minchin, F.R.S., gave the reasons which guided the Council in awarding the Linnean Gold Medal to Professor Otto Bütschli, of Heidelberg, and handed the Medal to him for transmission to the recipient.

Prof. Minchin briefly replied, expressing his pleasure in receiving this Medal on behalf of his old teacher and friend since the year 1888.

The General Secretary then laid his obituary notices of deceased Fellows before the Meeting, and the proceedings terminated.

OBITUARY NOTICES.

Though our late Fellow, Dr. Tempest Anderson, did not make any direct contribution to biological science, yet by his Presidency of the Yorkshire Philosophical Society and of the Museums Association, and by his generous bequests, he indirectly aided the departments of science for which our Society was founded.

Tempest Anderson was born at York in 1846, the eldest son of William G. Anderson, J.P., M.R.C.S., in the house at 17 Stonegate, which formed his home during the whole of his life, a wonderful old timbered house, in a narrow roadway leading towards the Minster, a fit residence for the descendant of an old Yorkshire family. A friend of his says: "The garden was a revelation to his guests, for the lawn reminded one of the turf in an old college quadrangle, and a fig-tree flourished under the study window."

He was educated at the School of St. Peter's, York, and University College, Gower Street, London, where he greatly distinguished himself and of which he was elected a Fellow. He graduated at London University as B.Sc., and took the M.D. degree at the same University. In 1904 the University of Leeds conferred upon him the honorary degree of D.Sc., of which he was very proud.

Returning to York, he entered upon medical practice, and, specializing as an oculist, became Consulting Ophthalmic Surgeon

to the County Hospital at York. As the years passed, and his leisure increased, he gave more time to his favourite geologic and mountaineering trips, and particularly to the phenomena of volcanoes, of which he succeeded in getting many striking photographs. In 1903 he brought out a volume, 'Volcanic Studies,' in which he mentions that for eighteen years he had spent the greater part of his holidays in exploring volcanic regions, including Vesuvius, Etna, the Lipari Islands, Auvergne (many times), the Eifel, the Canary Islands, and Iceland; in 1900 the Grand Cañon of Colorado and the Yellowstone Park.

In 1902 he visited the Soufrière and Mont Pelée in Guadaloupe as the accredited representative of the Royal Society with Dr. Flett, and in later years he examined the volcanoes of Guatemala and Mexico, Hawaii, New Zealand, and was in South Africa in 1905 with the British Association, as far as the Victoria Falls of the Zambesi.

Our former Zoological Secretary, Walter Percy Sladen, married his sister Constance, and after the death of both the Percy Sladen Trust was constituted upon a bequest from Mrs. Sladen, Dr. Tempest Anderson being one of the Trustees until his death. In 1912 he started for a long journey to Java, Krakatoa, and the Philippines, and when half-way home, he succumbed to an attack of enteric fever on 26th August, 1913, and was buried at Suez. He was never married.

As Secretary and afterwards President of the Yorkshire Philosophical Society, he was a moving spirit of that body, and presented to it a fine lecture-theatre, now known as the Tempest Anderson Hall, and when it was formally opened in June 1912, he was presented with his portrait painted by William Orpen, A.R.A., as a mark of the Society's appreciation of the work he had accomplished for it. When the British Association met at York in 1906, he acted as host every afternoon at the tea-tables in the beautiful grounds of St. Mary's Abbey, in which the Society has its rooms.

He left various bequests to relatives and friends, and directed that of the residue, about £75,000, two-thirds should go to the Yorkshire Philosophical Society and one-third to the Sladen Trustees, who are to add experimental physiology, pathology, and therapeutics to the list of subjects for their grants.

He was elected Fellow of the Linnean Society 15th March, 1906, and belonged also to the Geological and Royal Geographical Societies; in all three Societies he had served as Councillor.

[B. D. J.]

Carl Chun, who died at Leipzig on the 11th April, 1914, was born at Höchst am Main on 1st October, 1852. He studied at Göttingen and at Leipzig, where he was a pupil of the famous zoologist, Rudolf Leuckart, whose assistant he became in 1878. In 1883 he was appointed professor of Zoology in Königsberg, and in 1891 he went to Breslau, whence he was called, on the

death of Leuckart in 1898, to succeed to the chair of his old master in Leipzig.

Chun's graduation thesis (1878) dealt with the nervous and muscular systems of the Ctenophora, and was the first of a long series of memoirs, which have become classical, on these and other organisms of the marine plankton. The first of the well-known monographs, issued by the Zoological Station at Naples, was Chun's volume on the Ctenophora (1880), exquisitely illustrated by his own pencil. It was in this work that he first called attention to a remarkable phenomenon, which he later discussed at greater length (1892) under the name of "Dissogonie." Briefly stated, this consists in the occurrence, in certain Ctenophores, of two periods of sexual maturity in the life in the same individual, the one in a larval stage, the second after the assumption of adult characters. These two periods are separated by an interval, during which the genital organs return to a more embryonic condition, while the organism continues its metamorphosis.

In 1888 Chun was associated with Leuckart in founding the 'Bibliotheca Zoologica' (afterwards 'Zoologica'), perhaps the most lavishly illustrated of all zoological periodicals, of which he afterwards became sole editor. Notable among his own contributions to this journal are a series of memoirs on the bionomics of pelagic organisms, which appeared under the general title of "Atlantis." In these he discussed, among other subjects, the phosphorescent organs and the modified eyes of certain bathypelagic crustacea, and showed how the structure of the eyes was specially adapted for vision in a dim light.

In 1898–99 Chun was leader of the German Deep-Sea Expedition in the 'Valdivia,' of which he has given a most interesting account in his book 'Aus den Tiefen des Weltmeeres' (1st edn. 1902, 2nd edn. 1906). The scientific reports of this expedition, which have been in process of publication under his editorship, form a stately series of volumes.

Chun's influence as a teacher is strikingly commemorated in the 'Festschrift,' dedicated to him by his pupils on his sixtieth birthday and published as a volume of 'Zoologica.' The list of contributors includes many of the most distinguished among the younger zoologists of Germany, and the variety of the subjects dealt with is evidence of their teacher's interest in widely diverse branches of the subject.

Chun was elected a Foreign Member of the Linnean Society in 1904. He was a foreign member of the 'Challenger' Society and a corresponding member of the Zoological Society of London.

[W. T. Calman.]

Peter Ewing was born at Kinross on 13th July, 1849, but passed the greater part of his life in Glasgow; from his early youth he was an enthusiastic botanist, giving all his available leisure from business to the collection of plants. Specially attracted to the study of Alpine plants, he stated in 1911 that he had stood on

53 different occasions on the summit of Ben Lawers, and had spent more than 200 days upon that mountain.

He was elected a Fellow of the Linnean Society on 20th December, 1894, and from 1902 to 1905 he was President of the Natural History Society of Glasgow, in whose publications most of his contributions to botanic science appeared. He wrote several 'Contributions to the Topographical Botany of the West of Scotland' between 1886 and 1895; 'An Ecological Problem' in 1904, and his separate little work, a 'Glasgow Catalogue of Native and Established Plants' in 1892, which was revised in 1899—in this he catalogued 1959 species.

His visits to London rarely missed a call upon the Linnean Society, to consult books or specimens, even though the occasions of his journeys south upon insurance business demanded the lion's share of his attention. He died on 3rd August, 1913, in a nursing-home in Glasgow, after an operation. [B. D. J.]

By the death of Dr. Albert Karl Ludwig Gotthilf Günther on 1st February, the Society has lost an illustrious Fellow, who had for many years been one of its trusted counsellors. He was born on 3rd October, 1830, at Esslingen, Württemberg, where his father held the post of an Estates Bursar of the municipality. He attended the gymnasium at Stuttgart and studied in the Universities of Tübingen, Berlin, and Bonn. Destined originally for a career in the Lutheran Church, he soon devoted himself to Natural History and Medicine, and obtained the degree of Philosophiæ Doctor in Tübingen in 1852 and of Medicinæ et Chirurgiæ Doctor in the same university in 1862. In 1856 he paid a visit to England for the purpose of studying the zoological collections of the British Museum. This led in the following year to his temporary and subsequently (1862) his permanent appointment on the staff of the Museum. In 1872 he succeeded G. R. Gray as Assistant Keeper, and three years later (1875) Dr. J. E. Gray as Keeper, of the Zoological Department, which post he held until retirement in 1895. It was during his Keepership and under his direction that the vast collections of the Zoological Department were successfully moved to their new home (1882–1883).

An appreciation of Günther's work as zoologist and of his keen love of nature generally follows below. Here it may suffice to state that his first publications date back as far as 1853. They are a short article, "Ueber den Puppenzustand eines Distoma," a note "Beiträge zur Fauna Württembergs," and a memoir on "Die Fische des Neckars," all in the 9th volume of the 'Württemberg'sche Jahreshefte.' He was a fertile writer, and the titles of his papers in the Royal Society's 'Catalogue of Scientific Papers up to 1883'—that is, where the Catalogue stops—fill no fewer than 14 columns. His last contribution to science was "Andrew Garrett's Fische der Südsee," in Journ. Mus. Godeffroy, Heft 17 (1910) pp. i–vi, 389–515.

To appreciate his activity as administrator and organizer, we

have only to turn to his 'General History of the Department of Zoology from 1856 to 1895' (Appendix to vol. ii. of 'The History of the Collections contained in the Natural History Departments of the British Museum'), published in 1912 by the order of the Trustees of the British Museum. Written in the terse and sober language of the recorder, it testifies through its bare facts and figures to the loyal devotion and the organizing genius of the man who for nearly forty years took an ever-growing—and, finally, leading—share in the marvellous activity which the book unrolls.

Placed in one of the centres of scientific life, he could not, even if he had wished, escape the claims which the organized corporations of his fellow-workers were bound to make on one so learned, so judicious, so conscientious, and, above all, so utterly unselfish. So we see him elected a Fellow of the Royal Society in 1867 and a Vice-President of the same Society for 1875–1876; a Fellow of the Zoological Society in 1862, a member of its Council for 31 years, and a Vice-President for 24 years; a Fellow of the Linnean Society in 1877, a member of its Council for 10 years, and its President for 4 years (1896–1900); and in 1880 President of the Biological Section of the British Association. Nor did his merits remain unrecognized. The Royal Society awarded him its Royal Medal as early as 1878, and the Linnean Society the Linnean Medal in 1904; whilst learned societies of many countries enrolled his name in the lists of their members.

As to Günther's services to the Society, there is one important aspect which must not be passed over in silence. The first is the great amount of time and trouble he gave to the arrangement of the Linnean specimens of fishes, their preservation in glass-topped boxes, and a full catalogue of them, which will be found printed in our 'Proceedings,' 1898–99, pp. 15–38. The other was the acquisition for the Society of the entire Swainsonian correspondence, by the help of the late Hon. Charles Ellis, the Hon. Walter Rothschild, Sir Frank Crisp, Mr. F. D. Godman, and the Bentham Trustees. Besides being the active promoter of this purchase, Günther gave a complete catalogue of all the letters, with an introduction, setting out some of the more interesting items. This calendar was printed in our 'Proceedings' the year following the above-mentioned Linnean list, namely, for 1899–1900, pp. 14–61. The portrait chosen for the frontispiece depicts our former President reading the correspondence referred to; it was reproduced in collotype from a water-colour drawing by Miss Lucy Gee (now Mrs. H. Coxeter) in the possession of Mr. R. T. Günther.

It would not be in the spirit of the deceased to dwell on the aspects of his private life, in which he had his ample share of bliss and of sorrow. But the friend may be permitted to conclude this all too short sketch with a few words of tribute to Albert Günther, the man.

He had many friends. If from among his fellow-workers he

attracted them through the breadth and thoroughness of his knowledge, from among his official colleagues through his unflinching sense of duty and his fairness and sympathy, from among wider circles through a rare union of unaffected dignity and natural charm of personality, he held them all by the transparent sincerity of his character which was virile without harshness, generous to the point of self-oblivion, and true to the core. He gave freely and yet almost humbly from the wealth of his knowledge and experience, unconcerned whether he received anything in return or not. It was permitted to few to look into the depth of his soul. A chaste veil lay over it, born of humility and delicacy; but through it radiated all the same the warmth of a great and strong heart. There are noblemen by the grace of man, and others by the grace of God—and he was truly one of them.

He bore his years to the last apparently in robust health of body and mind, only his eyesight had of late begun to fail very seriously. Then came the end, short and swift, as if it had been decreed that he should be spared the trial of blindness, the severest that could have befallen him. Three days later he was buried at Richmond by the side of his beloved only daughter.

[O. Stapf.]

In Zoology the name of Günther stands high among the great constructive systematists and missionaries of organic method. His work is incorporated in the fabric of the British Museum, where half his days were spent, and where for twenty years he ruled as Keeper of Zoology.

It would be impossible for an outsider to do justice to his reputation as a Keeper; but all the world knows that it was he who accomplished the stupendous task of removing the zoological collections from Bloomsbury and establishing them afresh at South Kensington, and all who appreciate the value of circumstantial evidence attribute to him in incomparable measure both the inspiration and the impetus which have made those collections in their new home the easy and effective instruments of research that they now are. The one desire of his life was to see the Natural History Museum the recognized headquarters of the study of systematic zoology for the whole empire; and it is a simple statement of fact that when he was in the zenith of his powers the Natural History Museum and Dr. Günther were equivalent expressions to a host of naturalists and explorers scattered through the British Possessions.

As an authoritative systematist his influence was wide-spread, but for a ready illustration of his momentum in a specific direction, one naturally selects his great 'Catalogue of Fishes,' since, in his own words, he devoted the best years of his life to its execution. It is, in truth, a monumental work—not merely a massive achievement of steadfast and discerning labour, and a beacon in science, but an imposing standard of taxonomic method, and also an enduring model of a museum instrument. Its originality

lies in the fact that it is the first reference catalogue which is also a replete monograph adequately expressing a significant natural classification. It surveys the whole field of ichthyology, discriminating and defining, group by group, every order, family, and genus, and every one of the seven thousand species known at the time, whether represented in the Museum or not—the pervading idea being to make the collection not only accessible to specialists and students, but also generally illuminating, and furthermore perennially fruitful and recuperative to itself. No doubt, there may be some parts of the original fabric of the Catalogue that, in the light which it itself has helped so greatly to spread, may now seem somewhat worn: opinions will always fluctuate as to the limits of genera and families; with extension of knowledge views must necessarily change as to the phyletic significance of particular structures and organs; but, howsoever any of its contents may become modified by time, nothing can be effaced or lightly dismissed, and for quality and style, for coherent comprehensiveness and lucidity, Günther's Catalogue will always stand out as a great original exemplar. It is hardly possible to overestimate the stimulating influence that it and his 'Introduction to the Study of Fishes'—another work of conspicuous originality,—together with his "Report on the 'Challenger' Deep-Sea Fishes," have exercised in this one particular branch of science.

But even as a systematist Günther's influence extended far beyond the field of ichthyology.

In herpetology he was hardly less authoritative and stimulative, and his 'Reptiles of British India' is another of the big landmarks of natural history. It, and also his 'Catalogue of Batrachia' and 'Catalogue of Colubrine Snakes,' have been superseded for practical purposes in the onward movement of science, but time cannot stale their merit as foundation-work, nor wither their rooted credit as incentive and directive forces in that progress.

In the history of systematic zoology Günther will also long be held in remembrance as one of the inceptors and, for the first six years of its existence, the editor of the 'Zoological Record,' and as for nearly forty years one of the editors of the 'Annals and Magazine of Natural History.'

To portray Günther only as a great museum-administrator and systematist would be to give a very incomplete impression of him as a zoologist.

His paper on *Ceratodus*, published in the 'Philosophical Transactions of the Royal Society,' ranks as a classic in Comparative Anatomy, for it remains a great original storehouse of formulated facts of structure, even though all the phyletic interpretations of fact have not been established.

But beyond everything he had a perfect heart towards Nature, and was an observer of the humanest and most universal temper, literally overflowing with delight in all things having life. For

birds he had a peculiar affection, which they, whether caged in his conservatory or free in his garden, reciprocated in ways that were wonderful to witness. It was no mere accident that one of the last papers that he published dealt with the habits of certain native birds.

It was this perennial interest in living creatures that made him so effective in his relations with travellers and explorers, and that miscellaneous multitude of people interested in natural history who are only too willing to bring all their opportunities into the common service if they can get in touch with some one to direct and support them on their own level. It is natural to infer that this too had a good deal to do with that breathing of life into dry bones which was one of the miracles worked in the Natural History Museum soon after its new foundation in Cromwell Road.

From the esoteric tendencies of modern taxonomic zoology, with its fine-spun distinctions, its rather aberrant dialectics, and its idolatry of the museum type, Günther stood aside. To him a species was not a defunct entity enshrined with an inscription in a cabinet, but a group of living things with natural affinities ; and in the investigation of these affinities—which he, with Agassiz, considered to be the main business of the systematist—he held consistently to the belief that long monotonous descriptions of individual specimens are more likely to mislead than short differential diagnoses, where the salient specific features are shown in strong relief.

Though he never wrote of museum organization as an independent theme—for anything savouring of self-importance was quite alien to his nature,—he naturally had formed critical judgments on the subject; and the authorities of the British Museum are much to be congratulated that, in entrusting to him the preparation of their lately-published 'General History of the Department of Zoology,' they afforded him an opportunity of incidentally recording some of those judgments in an official document.

Though he never talked of the public educational value of a museum of natural history, no man had surer instincts or clearer insight in this matter also. In the theory of long descriptive labels illustrated by specimens, he put no trust, divining that the educational opportunity of a people's museum is to reach the vacant mind through the objects themselves. [A. Alcock.]

Adolphus Henry Kent was born at Bletchingley, Surrey, in 1828, and was educated for a schoolmaster privately. Becoming the schoolmaster of his native village, he remained there for the greater part of his life. Sir George Macleay, of Pendell Court, knew and appreciated Kent's knowledge of plants and his love of gardening, which he pointed out to Mr. (now Sir) Harry Veitch one day.

Matriculating at the University of London in 1870, he obtained

his degree of B.A. in the following year, being placed in the first division.

He was obliged, soon after this, to give up his position as schoolmaster on account of growing deafness, but his former acquaintance with the leading partner of the firm of Messrs. James Veitch & Sons, of Chelsea, was of the greatest value to him, as he became private secretary to the head of the firm, and for many years he was occupied in drawing up works of very great value which were published by that firm. We may name 'A Manual of the Coniferæ,' published in 1880, and reaching a second edition in 1900; another work, the outcome of long-continued labour, is 'Manual of Orchidaceous Plants cultivated under Glass in Great Britain' in ten parts—the first, devoted to the genus *Odontoglossum*, appeared in 1889, the last, in 1894, gave a general review of the Order.

He was elected an Associate of this Society on the 5th May, 1889, and made constant use of our Library. Upon his retirement, a few years since, upon a liberal pension, he still enjoyed borrowing books on the various subjects connected with botany and gardening. His own large collection of books he bequeathed to a nephew, as he had no children and his wife predeceased him. He died at his house, "Mycene," Doria Road, Fulham, on the 12th September, 1913. [B. D. J.]

The Society has the misfortune this year to record the death of two former Presidents in the same year—Lord Avebury and Dr. Günther.

The Right Hon. Sir John Lubbock, 4th Baronet and 1st Baron Avebury, was born in London on the 30th April, 1834, the eldest son of Sir John William Lubbock, 3rd Baronet, who was well known as a physicist and mathematician. After education under tutors, he went to Eton, and was there simultaneously with four younger brothers. He left Eton at a very early age, losing all chance of a University training, in consequence of the serious illness of some of the partners in his father's bank. Thus, at the age of 14, he made his first acquaintance with the routine of the banking business of Sir John Lubbock, Edward Forster, and George Hotham, which afterwards became Robarts, Lubbock & Company. At the time young Lubbock entered business, one of the partners was Edward Forster, as mentioned above, and this man was Treasurer of the Linnean Society from 1816 to 1849, a long period of 33 years. There is no record of the influence Forster may have exercised upon the youth at the impressionable age, but as the elder man was of considerable note in botanical circles, and possessed a fine herbarium, it is not perhaps unduly hazardous to speculate that he may have had a considerable share in directing young Lubbock's attention to natural history. In passing, it may be pointed out that Forster's herbarium was acquired by the British

Museum, and formed the basis of the present British Herbarium now at Cromwell Road.

Another influence—and, without doubt, a greater one—was that mentioned by Lord Avebury at the Darwin-Wallace celebration on the 1st July, 1908. Speaking of Charles Darwin, our late Fellow said:—"I first heard his name in 1842—66 years ago—when my father returned one evening from the city and said he had a great piece of news for me: Mr. Darwin was coming to live close to us at Down, which he has rendered so famous."

Mr. Lubbock joined our Society on the 21st January, 1858, at the age of 23, and compounded for his annual payments, an investment which has hardly ever been equalled, for during 55 years the use he made of the library has not been approached by any other recorded Fellow. How many of his facts in his long series of papers and books were derived from volumes in possession of the Society can never be ascertained. Soon after his admission, his first paper, on the "Cutaneous Muscles of the Larva of *Pygæra bucephala*," was printed in the 22nd volume of our 'Transactions,' followed by twelve papers in the same set of volumes, besides others in our Journals. All his 'Transaction' papers were on insects and many of the Journal papers also; but he afterwards broke ground by botanical observations. Naturally, it is his biological papers which appeal to us, but the general public know him as an author chiefly by his popular works, such as "Use of Life," "The Beauties of Nature," "The Pleasures of Life" in two parts, "Prehistoric Times," "The Scenery of Switzerland," and the like, some of which ran into very many editions and were translated into most of the European languages and even into Japanese.

But, side by side with his scientific studies, his business pursuits and political life caused him to publish essays and addresses on points of interest for the passing moment, and also on archæological and anthropological matters. It was his many-sided existence which attracted the notice of the public, and that regard was finally fixed when the Bank Holidays Act was passed in 1871, which added the first Monday in August to the days following: Boxing Day, Easter Monday, and Whit-Monday.

Sir John Lubbock, who had succeeded to the baronetcy in 1865, was elected President of the Linnean Society in 1881, and made his influence at once felt. His predecessor was far better as an observer and describer than as an administrator, and under his indulgent rule the debates were suffered to become discursive and irregular. The new President quickly brought his trained faculties to bear, and the conduct of business became both quicker and more regular. He also revived the receptions, which had been dropped since the days of Mr. George Bentham, and the five years of his presidency were notable in the history of the Society. Upon his quitting the Chair, the present rule was established with his entire sanction, by which no president is elected more than four times.

After this period, although it was rarely that Sir John Lubbock appeared at any of our meetings, and then only to read one of his papers, he frequently visited the library, or still more often sent his secretary for volumes on subjects upon which he was working. This he continued until a very short time before his death, which happened on the 28th May, 1913, four days after our Anniversary. He died at his seat, Kingsgate Castle, near Ramsgate, but was buried from his other Kentish mansion, High Elms, near Orpington; he was carried on the shoulders of his tenants from that house to the churchyard of Farnborough.

His honours were many and varied; he was sworn of the Privy Council in 1890, and created first Baron Avebury in 1900, taking his title from his Wiltshire estate, a small village, containing the site of what is the largest Druidical temple in Europe, though ruder and less known than Stonehenge; of the 650 stones formerly standing, only about 20 remain, and many of those which have been carried off have served as building-material for the village. Aubrey stated that Avebury "did as much for Stonehenge as a cathedral does a parish church." When some years before his elevation to the peerage it was proposed to destroy this venerable relic and to build upon the site, Lord Avebury averted the calamity by purchasing the estate.

He was Commander of the Legion of Honour and the Prussian "Ordre pour le Mérite"; had been President of the Society of Antiquaries, Royal Microscopical, Entomological, Ethnological Societies, and Anthropological Institute, and of the British Association Jubilee Meeting at York in 1881. At the time of his death he was President of the Ray Society and of the Selborne Society, together with many other Associations and Committees, too numerous to particularise, and was Vice-President of many others. The citizens of London appreciated his qualities, and he occupied the Chair of the London County Council during 1890–92. His Parliamentary career began in 1870, when he became Member for Maidstone, retaining the seat until 1880, in that year being elected for the University of London, which he represented until his elevation to the House of Lords. In both Houses he attended assiduously, and a curious sign of this lies in the fact that many of his papers were written on House of Commons note-paper, on doubt jotted down whilst waiting for the division-bell. Indeed, his manuscripts were singular in this, that while most was written on the Commons paper, intercalated were sheets of club note-paper or with private headings, showing that, having determined his procedure, the actual writing could be accomplished anywhere.

His speaking was characteristic of the man, moderate and even, with a tranquil flow, far removed from strong feeling or passion. Attached as he was to all things he had taken up, he needed to satisfy himself about every new departure, not only that it was desirable, but almost certain to succeed, before he identified himself with it. It was not in his well-balanced temperament to espouse a lost cause or to fight a losing battle. The advocacy

of Proportional Representation is really a case in point, for its justice and even-handed plan commended itself to his well-regulated mind.

As an author the extent of his compositions is great, and the more so when it is recalled that his banking business in the day-time, and parliamentary duties afterward, took up so much of his time. The secret of it is that he had thoroughly mastered the art of devolution, and made extensive use of others in his various publications. Ingenious and apt in devising experiments, he largely committed the task of observation to others, while he himself was busy in the city. Possessed of an easy and fluent pen, he was accustomed to submit his manuscript and proofs to authorities in their respective departments. Thus while the facile writing pleased the general reader, the author had ensured the absence of palpable error, and so appealed to the man of Science. The Presidential Address at York in 1881 is a good instance of this composite writing; in reviewing the progress of science during the previous half-century, he was naturally compelled to enlist the help of workers in other fields than those known to him.

One feature of his social life must not be omitted mention—the breakfasts he gave at his town house during the London season, resembling those of Sir Joseph Banks and the banker-poet, Samuel Rogers. The time was half-past nine, about a dozen to twenty was the usual number of guests, and none who were invited to these gatharings will forget their pleasant charm.

The portrait of our former President which hangs in our Meeting Room was painted for the subscribers by Mr. Leslie Ward, from no fewer than 27 sittings.

Lord Avebury was twice married: first to Ellen, only child of the Rev. Peter Hordern, in 1856, who died in 1879, by whom he had three sons and three daughters, and second to Alice Augusta Laurentia, daughter of Lt.-Gen. A. H. Lane Fox-Pitt-Rivers, who survives, and by her had two daughters and three sons.

[B. D. J.]

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[Address to the Selborne Society on Botanic Observation.] *L. c.* xv. (1904) pp. 136–140.

On the Forms of Stems of Plants. Rep. Brit. Assoc. 1904 (1905), pp. 812–813, 2 figs.

Although Sir John Murray was very near to seventy-four years of age at the time of the accident in which he lost his life, he had not commenced to show any failure of either bodily or mental powers. Only a few months before the fatal event he had given to the public a handbook upon Oceanography, and his obviously strong constitution showed no signs of being impaired. This robustness of body induced sane work, and the characteristic of Murray's contributions to science reflect this association. Immense application in the collecting and formulation of details of fact, coupled with perfectly clear deduction, uninfluenced by any desire to produce unnaturally strained effects, is the distinguishing feature of his work. The researches conducted by Murray were, as is well known, entirely connected with the geology of oceans and lakes; and the most important of all his conclusions is, of course, that relating to the formation of coral reefs, in which he revolutionised opinion current at the time.

This line of work was initiated by Murray's appointment in 1873 to the naturalist staff of H.M.S. 'Challenger.' The zoological side of the expedition was amply cared for by the director of the scientific staff, the late Sir Wyville Thomson, and by his two assistants, the late Prof. Moseley and the late Dr. v. Willemöes-Suhm. It was naturally therefore left to Murray to devote himself to the geological aspects of the dredgings carried out during the voyage. For this line of work he had been well prepared by his studies at the University of Edinburgh under Sir Archibald (then Professor) Geikie, the late Prof. Tait, and Prof. Crum Brown. Extensive observations were made during the voyage, and, on the conclusion of the expedition, Murray, in conjunction with the late Prof. Renard, published one of the series of the 'Challenger' volumes upon these deposits. The same kind of work was pursued in after years by numerous minor exploring expeditions, and, latest of all, Murray commenced with the assistance of others a survey of the Scottish lakes with their fauna and deposits, providing the necessary finances. The facts gathered in this survey fill six volumes, of which it has been said that "there is probably no other country of which the depths and other features of its lakes have been so fully made known."

Although one of the leading, and at the time of his death probably *the* leading, enquirer in this department of geology, Murray was a most wide-minded man in his views of other branches of science; he was anxious to assist in purely zoological and botanical work with the same fervour that he devoted to his own studies. His founding or patronage of zoological stations in Scotland (such as the laboratory at Granton) are well known; and with him were associated many young naturalists, at that time unknown, but now well known for their contributions to science (such as Mr. J. T. Cunningham, Dr. H. R. Mill, and many others).

A very marked part of Sir John Murray's character was his encouragement of, and assistance to, younger men desirous of pursuing scientific paths; nor was this encouragement tainted by the least trace of jealousy, so lamentable a trait in the character of even some very great men of science of the past. There are many now with us who have much reason to be grateful to Sir John Murray.

After the return of the 'Challenger' Sir John Murray assisted Sir Wyville Thomson; and, on the death of the latter in 1882 succeeded him in the editing of the vast series of volumes in which the scientific results of the Voyage were set forth, and in the conduct of the huge collections. He himself, as already stated, was responsible with Prof. Renard for that part dealing with the marine deposits and also, with assistance from the late Prof. Moseley and some others, for the writing of the introductory part dealing with the cruise itself and containing *résumés* of the scientific work done. The editing of so colossal a series of volumes is most justly to be credited to Sir John Murray, who supervised every matter himself and read proofs with the same diligence that he devoted to other departments of his duties to

H.M. Treasury. In selecting his contributors—the selection being virtually in his hands—Sir John Murray was careful to gain the services of certain of those already distinguished for their knowledge of the various groups of animals which were offered to them for their study. But this very necessary part of his duty did not prevent Sir John from generously helping many younger men by including them in the list which contained some of the greatest names in Europe (such as Haeckel, Kölliker, Agassiz, M'Intosh, F. E. Schulze, Günther, &c.). He was particularly kind in this matter to his own countrymen, though he also secured the services of some naturalists from abroad of the then young generation. It was unquestionably due to Murray's energy of character and justly earned and general popularity that enabled the completion of this monumental work, which has never been equalled in the mass of accumulated and novel information by any subsequent exploring voyage. This must, of course, be regarded as Murray's chief life-work on the official side, and it is gratifying to reflect that he received many testimonies to its importance by the numerous honours which were subsequently conferred upon him.

Born at Coburg, Ontario, on the 3rd March, 1841, he died in consequence of a motor-car accident near Edinburgh on the 6th March, 1914. He was elected a Fellow of this Society the 15th November, 1883. [Frank E. Beddard.]

Thomas Hawkes Russell, who died at Edgbaston on 31st July, 1913, was born on 30th March, 1851, the eldest son of the late Dr. James Russell. Educated at the Birmingham and Edgbaston Proprietary School and University College, London, he adopted the profession of the law, and practised as a member of the firm of Lee, Musgrove, & Lee, Solicitors, until 1905, when he retired and devoted his leisure to local activities and botanic studies. He was elected Fellow on the 1st March, 1906, and in 1908 he published a small octavo volume, 'Mosses and Liverworts: an Introduction to their Study, with Hints as to their Cultivation and Preservation,' which was issued by Low & Co., the 10 plates being from the author's own drawings; a slightly enlarged edition came out in 1910.

During the last eight years of his life he gave himself energetically to lecturing on botanical subjects in the neighbourhood of his home, his diagrams being enlarged from his coloured drawings of objects under the microscope.

After cremation his ashes were interred at Witton Cemetery on the 5th August last. [B. D. J.]

Philip Lutley Sclater.—By the death of Philip Lutley Sclater, which took place on June 27th, 1913, the zoological world lost an extraordinarily able leader and the Linnean Society one of its most distinguished Fellows.

In how far the trend of his life's work was influenced by the

difficulties which beset those who, in his early days at Cambridge, were possessed by an insatiable thirst for knowledge in regard to all that pertained to Natural History, we shall never know. In his day there were no organized Science Schools, and Biology found no part in the teaching scheme of the University. Hence, like Darwin and others of his day who in after life attained to fame as zoologists, he had to satisfy his cravings as best he might. But it was to these limitations, probably, that Systematic Zoology and the Geographical Distribution of Animals alone occupied his post-graduate life. To these he made splendid contributions. He, indeed, was the founder of the science of Zoo-Geography; and if he had done no more than this, he would have earned his niche in the Temple of Fame.

Among ornithologists his name was revered, and his contributions to science loom largest in the enormous output of ornithological memoirs which stand to-day to his credit. The bulk of these were purely systematic in scope, but he wrote one or two small papers on the wings of birds and devised a scheme of Classification. The latter, however, from his lack of anatomical knowledge, added nothing to his fame.

His activities were, however, by no means confined to birds, for he possessed an encyclopædic knowledge of mammals from the systematist's point of view. But Sclater's influence on the zoological world was by no means limited to his published writings. His personal influence was enormous, subtle, and far-reaching. It made itself felt in no uncertain way in the affairs of the Zoological Society of London; for he controlled its destinies for forty-three years, and made it famous throughout the world.

These concrete fruits of a long and useful life are matters of common knowledge; they are historical facts, which those who will may estimate according to their own conceptions of merit. But no measure can be applied to his influence on the men of his time, and especially the younger men. He was a keen judge of character, and zealous in his efforts to win recruits for his beloved science. None who appealed to him for help or advice ever turned away empty. But he did not always wait to be asked: whenever he met with promising youngsters, he was certain to offer his aid to promote their welfare. Many men who have attained to eminence owe much to him.

That he was held in high regard by the men of his time is shown, in the first place, by the fact that Darwin, Owen, and Huxley were among his closest friends, and, in the second, by his success in promoting expeditions to unknown lands charged with the task of exploring their fauna and flora.

His work for Societies other than his own was considerable. For many years he edited 'The Ibis,' the Journal of the British Ornithologists' Union, and the 'Journal of the Ornithologists' Club.' Of the latter he was Chairman from its inception in 1892 till his death. For many years he acted as Secretary to Section D of the British Association for the Advancement of Science, and in 1875, at the Bristol meeting, was its President. That occasion

was made memorable by his brilliant address "On the Present State of our Knowledge of Geographical Zoology." He twice served on the Council of the Royal Society.

Among his recreations, perhaps, we may reckon his work as a Justice of the Peace; and he was also a keen huntsman, following the hounds up to the day of his death, which was due to a carriage accident.

The science of Zoology owes not a little, perhaps, to the fact that in those early days its territories had to be explored without the aid of guides. This served to concentrate attention on faunistic zoology, and to lay the foundations for the study of Geographical Distribution, which has so important a bearing on the history of the zoology of past ages. Sclater, as we have already remarked, was a pioneer in this field, and if this had been his only achievement it would have been a great one. [W. P. Pycraft.]

Mr. Martin John Sutton was born in the year 1850, the eldest son of Martin Hope Sutton, of Whitley, Berks. He became the senior partner in the firm of Sutton & Sons, the Royal Seed Establishment, Reading, and his activities were directed almost entirely to the advancement of horticulture and agriculture. The dignities conferred upon him are proof of this: he was Chevalier of the Legion of Honour and of the Ordre du Mérite Agricole; Knight of Grace of the Order of the Hospital of St. John of Jerusalem, Member of the National Agricultural Examination Board, Governor and Member of the Council of the Royal Agricultural Society. He also took his share in the conduct of the Bath and West of England and Southern Counties Societies. Of local honours conferred, he possessed the Honorary Freedom of Reading, was J.P. of that town and of Oxfordshire, and County Councillor of Berks. He was twice married, and leaves a widow and two sons and a daughter.

He was elected Fellow, 3rd June, 1886; he died unexpectedly at the Piccadilly Hotel, London, on the 14th December, 1913, under an anæsthetic for a minor operation. [B. D. J.]

Alfred Russel Wallace, O.M., D.C.L., F.R.S.—The death of Alfred Russel Wallace on November 7, 1913, broke the last link with the band of illustrious Englishmen who made Evolution the watchword of the latter half of the nineteenth century—Darwin, Hooker, Huxley, Spencer, and Wallace himself.

The details of Wallace's long, happy, and varied life are so well known that it would be inappropriate to repeat them on the present occasion. I prefer to speak of his special association with the Linnean—to him, there is reason to believe, the dearest of all scientific societies. He was proposed in 1871, the Form of Recommendation, first read on November 2, being signed by Bentham, Stainton, Hooker, Newton, Flower, Gray, and six other Fellows. The election took place on January 18, 1872. But Wallace's connexion with the Society was much earlier than this; for, in addition to the epoch-making papers in 1858, he had

read his important memoir on "Mimicry in the Malayan Papilios" in 1864. Wallace's share in the joint papers was his greatest scientific achievement; while, in the second publication, he was the first to extend the principles set forth by his old fellow-traveller, H. W. Bates, to the East, and to discuss and throw new light on certain important aspects of Mimicry which had up to that time escaped attention—I specially refer to polymorphism in Mimicry and to the presence of Mimicry in the female. Not only was the parent theory of Natural Selection first brought before the scientific world by the Linnean Society, but the daughter theory of Mimicry, which formed and still forms its most striking illustration and defence, was first made known by Bates, then extended by Wallace, and later by Roland Trimen, through the same channel. Mimicry always possessed an intense fascination for Wallace, and I remember how he wrote to me soon after my election as Hope Professor, urging that the whole resources of the Hope Department should be devoted to this subject.

The last paper communicated by Wallace to a scientific society was read by him before the Linnean on June 18, 1896. The subject was "The Problem of Utility," and, as he has himself explained *, his "purpose was to enforce the view that all specific and generic characters must be (or once have been) useful to their possessor, or, owing to the complex laws of growth, be correlated with useful characters. . . . I endeavoured to show that the problem is a fundamental one, that utility is the basic principle of Natural Selection, and that without Natural Selection it has not been shown how *specific* characters can arise."

I recall with the greatest pleasure meeting Wallace at breakfast at Prof. Meldola's the morning after the meeting. He showed not the slightest trace of fatigue after the journey and the effort of reading the paper. The party, which also included Francis Darwin, sat and talked until far into the morning. Finally, as Wallace rose, he said, with the greatest animation, "Well, I should like to go on in this way all day!"

It is a piece of remarkable ill-luck that the Linnean Society does not possess a noble representation of one of our greatest Fellows—a splendid companion to the Darwin we know and love so well. Ten years ago the Hon. John Collier generously offered to paint a portrait of Wallace, and even, if it were necessary, to stay at Broadstone for the purpose. I told Wallace of the offer, and tried to persuade him to accept it. Others tried also, but no one could induce him to sit. "My portrait is just appearing in 'Black and White,'" he said, "and I am sure nothing could be better than that." The Linnean Society and the whole world of science and letters are the poorer for his decision.

Nothing would have pleased Wallace better than the thought that the books in his library, which bear the imprint of his personality, should find the permanent home in the Linnean Society

* 'My Life' (London, 1905), ii. p. 215.

now assured to them by the generosity of Mr. T. H. Riches. Many years ago he sent me E. D. Cope's 'Origin of the Fittest,' saying that he had read it and had no further use for it. I suggested that he should present it to the Library of the Hope Department, but he would not consent until he had first ascertained that the Linnean Society already possessed a copy. I well remember, too, how cordially he agreed with my suggestion that G. W. Sleeper's booklet should be placed in the Linnean Library.

The culmination of Wallace's life, July 1, 1908, was also intimately bound up with the Linnean Society. The admirable Memorial Volume, issued by the Society, makes it unnecessary to refer to the events of that great day, which will assuredly live as an abiding inspiration in the memory of everyone who was fortunate enough to be present.

Wallace himself, although he was eminently sociable and delighted in converse with his friends, was strongly averse to publicity and ceremony of every kind; and I think that his appearance on that momentous anniversary was due to a sense of duty, and not from pleasure. He felt that it was a unique opportunity of paying homage to the mighty genius whose name had been, and will ever be, associated with his own *.

[E. B. Poulton.]

[The subject of the foregoing memoir was born at Usk, in Monmouthshire, 8th January, 1823, and was therefore in his 91st year at the time of his death.]

William West.—Algology has lost its foremost British exponent by the death at Bradford, on the 14th May, 1914, of William West. He was a native of Leeds, where, on the edge of Woodhouse Moor, he was born on the 22nd of February, 1848. He studied for the Pharmaceutical profession, proceeding to registration on the 16th February, 1870. He removed to Bradford, and set up in business there in 1872. Two years later he was married to Hannah Wainwright, also a native of Woodhouse Moor, Leeds, and they had two sons and a daughter; Mrs. West died in 1904, after a long and tedious illness. All the children inherited the paternal ability, the two sons passing through Cambridge University with distinction and applying themselves to botanical work. The elder son, William, died at Mozufferpur in Bihar in 1901, very soon after taking up a biological research appointment; and the younger, George, is now Professor of Botany at the University of Birmingham. The subject of this notice in 1886 took up science teaching professionally on being appointed Lecturer in Botany at the Bradford Technical College, afterwards adding the teaching of Biology and Pharmacology.

* See also the Obituary by the present writer in 'The Zoologist,' Dec. 1913, p. 468, parts of which have been adapted for incorporation in the above notice.

Botanically, he was a man of a remarkably wide and varied range of knowledge. His acquaintance with all groups of plants, more especially of the whole of the Cryptogamia, was extraordinary; and his knowledge was not merely wide, but profound and detailed.

In the early period, as a general botanist, he took part, along with numerous other able naturalists of the county, in making the Yorkshire Naturalists' Union the remarkably powerful and successful instrument of local scientific research which it has ever since been. In 1880 he was Secretary of its Botanical Section; but the increasing claims of his teaching work prevented him from attending after this many of the meetings. But in 1899 he was elected President of the Union—significant evidence of the respect in which he was held by his fellow Yorkshiremen. About this time—1878 to 1887—he published numerous notes and short papers on general botanical subjects; and in 1888 he contributed a large amount of material to Mr. F. Arnold Lees's 'Flora of West Yorkshire.'

He now began to concentrate his energies on the freshwater Algæ, and particularly upon the Desmidiaceæ—a group which he speedily made peculiarly his own. He studied them from all points of view—systematically, morphologically, ecologically; and his vacations were systematically devoted to field-work in many parts of the British Isles, particularly in the remoter and more inaccessible parts of the mountain regions of England, Wales, Scotland, and Ireland. The results were seen in the publication of numerous important papers, some in the issues of the Linnean Society. Beginning near home, their (for his son George was now growing up and actively co-operating in the algological investigations) native county of York was adequately worked, papers published for each of the Ridings, and, finally, in 1900-01, a full Alga-flora of the whole county. Papers were also published dealing with North Wales (1890), the English Lakes (1892), the West of Ireland (1892), Scotland (1893), the South of England (1897), the North of Ireland (1902), and the Orkneys and Shetlands (1912). A couple of papers also appeared dealing with Denmark (1891) and Portugal (1892).

Material was now being sent to West and his son from various parts of the world, and this formed the basis for a number of memoirs. These included papers dealing with the American States of Maine (1888, 1891) and Massachusetts (1888), and for the West Indian Islands (1894, 1899). For the Old World were published memoirs dealing with Singapore (1897), Koh Chang (1901), Ceylon (1902), Burma (1907), and North Borneo (1914). Meanwhile, another able Leeds desmidiologist, Mr. W. Barwell Turner, had monographed the Desmids of India. The Wests dealt with Madagascar in 1895, Central Africa in 1896, and Welwitsch's African material in 1897; and in 1911 they published the freshwater Algæ collected by the Shackleton Antarctic Expedition.

Besides these systematic papers, numerous notes were published in various journals, as well as memoirs on the "Conjugation of the Zygnemaceæ" (1891) and "Observations on the Conjugatæ" (1898).

Speaking broadly, the earlier papers (to 1893 or so) appeared as by William West, most of the later ones as by himself and his son jointly; but the actual co-operation in the work had extended over the whole period of investigation, and of later years the algological work fell to the son, the father devoting attention to ecological studies of bryophytes and lichens, his latest paper—and one intended to be the first of a series—being read to the Linnean Society a month after his decease.

The algological work above summarized did not by any means exhaust the potentialities of the subject, and led up to a systematic investigation on another line—that of the Phytoplankton of lakes and rivers. In this the two Wests were the pioneer workers in Britain, and they set about their task in characteristically ample fashion, commencing the detailed field-work about 1900, devoting the vacation months of several years to the exploration of the lakes of Western and Southern Scotland, Northern England, Wales, Western and South-western Ireland, as well as Lough Neagh, Malham Tarn, and the rivers Ouse, Lochay, and Bann. The results achieved were of high importance, showing that the Phytoplankton of lakes in extreme Western Europe, and particularly the British Isles, differed completely from those of Central Europe by the presence and dominance of Desmids, and were quite a revelation and surprise to European algologists.

The culmination of the algological work was the publication by the Ray Society of the first four volumes (1904, 1905, 1908, 1911) of the six of their Monograph of the British Desmidiaceæ.

Such was the work accomplished by William West. Of the man himself it remains to be noted that his personality was such as to endear him to everyone with whom he came in contact. As a science teacher he was remarkably successful, and he enlisted the affections of his students in a most extraordinary degree. As a field-naturalist he was an ardent lover of Nature in all her aspects, and he was a man of warm enthusiasms, with a singular charm of manner and a vein of quiet genial humour; so that he was most beloved by those who (like the present writer) knew him longest and most intimately.

He was elected a Fellow of this Society on 17th March, 1887, and was the only one resident in Bradford at the time of his death.

The most characteristic portrait of West is the one published in the 'Journal of Botany' for July 1914, along with a memoir by the present writer. A much fuller memoir (also by the present writer) is published in 'The Naturalist' for July and August 1914, also illustrated by a (different) portrait, and to this memoir is appended a bibliographical list of West's principal papers and notes. [W. Denison Roebuck.]

June 4th, 1914.

Prof. E. B. Poulton, F.R.S., President, in the Chair.

The Minutes of the Anniversary Meeting of the 25th May, 1914, were read and confirmed.

Mr. Edward Ashdown Bunyard was admitted a Fellow, and Mr. James Lomax an Associate.

Miss Sarah Martha Baker, D.Sc. (Lond.), Mr. Charles John Bond, F.R.C.S., and Mr. Stuart Hogg, were elected Fellows.

The President announced that he had appointed the following to be Vice-Presidents during the ensuing year:—Prof. W. A. Herdman, Prof. E. A. Minchin, Mr. Horace W. Monckton, and Mr. H. N. Ridley.

The Rev. George Henslow, M.A., F.L.S., gave an address on "Darwin's Alternative Explanation of the Origin of Species, *without* the Means of Natural Selection," and supplied the following abstract:—

The *cause* of Variation, always "changed conditions of Life," with "Definite *or* Indefinite Results" (*Variation, &c.* vol. ii. p. 272, 1868).

Natural Selection is *not* a Cause: *e.g.* Mivart (*Origin, &c.* 6th ed. ch. vii.). Also, F. W. Hutton, who says:—"Having proved Natural Selection to be a *true cause*, and one that largely explained the Origin of Species from Varieties, by *causing* a gradual divergence of character, &c." (*Darwinism and Lamarckism*, p. 38, 1899).

No necessity for the words "Several generations" for giving rise to a variation, as plants *vary at once*; but they *are* required for fixing them so as to be hereditary under any conditions. "Species" and "Variety" are terms representing the varying amounts of change necessary for adaptation. Hence there need be no intermediate forms.

First reference to "definite action," &c., as a *Cause* (*Origin, &c.* 1st ed. End of Introduction, 1859). His original view (*Origin, &c.* pp. 11, 12) not strongly emphasized until 1868 (*Variation, &c.*, and *Origin, &c.* 6th ed. 1878). Reason for delay given to Wagner (*Life &c., &c.* vol. iii. p. 158; Oct. 1876), viz.:—"I could find little evidence of the direct action of the environment."

"Definite variation" leads to "permanent modification of

structure," *i.e.* Inheritance of Acquired Characters (*Origin, &c.* 6th ed. p. 421, 1878). These may become only *relatively, or* even *absolutely* stable under all conditions of life.

Indefinite Variations, *also caused* by "Direct Action of Changed Conditions of Life." They consist of a supposed mixture of individuals, the *minority* possessing "favourable" (*i.e.* adaptive), the *majority*, "injurious" (*i.e.* inadaptive) variations. Such is the *Theory of Natural Selection* as described (*Origin, &c.* 6th ed. p. 63, 1878).

Natural Selection is *not* required, for the majority die by "fortuitous destruction" (*Origin, &c.* 6th ed. pp. 53, 54, 59–89). *E.g.* Sir E. Ray Lankester says, that one out of a million eggs of an oyster may survive, *i.e.* per *chance* (Darwin's word).

Contra, F. Buckland says of young oysters transferred to new localities, that within two months they begin to assume the 'native characters' (*Variation, &c.* vol. ii. pp. 280, 281).

Some Illustrations:—Mesophytic plants becoming aquatic and *vice versâ*, *e.g.* Water Crowfoot. Monocotyledons originated from aquatic Dicotyledons. Mesophytic plants becoming Xerophytic and *vice versâ* by means of water, *e.g.* Restharrow, &c. Cultivated plants originating, by prepared soil, from wild plants, and their reversion, *e.g.* root crops. Depauperized and dwarfed plants, recognized as specific; through drought, submergence, parasitism, and saprophytism.

Darwin's final charge against scientists for their misrepresentations:—"Great is the power of steady misrepresentation; but the history of science shows that, fortunately, this power does not long endure" (*Origin, &c.* 6th ed. p. 421). It has lasted for fifty-five years (1859–1914).

Dr. Rendle, Mr. A. W. Sutton, and the President joined in a discussion, and the author replied.

Mr. Guy C. Robson, B.A., gave an abstract of his paper "On a Collection of Land and Freshwater Gastropods from Madagascar, with Descriptions of a new Genus and new Species" (communicated by Prof. G. C. Bourne, F.R.S., Sec.L.S.). The affinities of the species examined were found to be mainly Oriental and not African.

Mr. James Lomax, A.L.S., showed series of sections of the entire vertical thickness of a seam of coal, also shown by photographs of sections in the lantern.

Prof. H. H. W. Pearson, F.L.S., contributed a paper, "Notes on the Morphology of certain Structures concerned in Reproduction in the Genus *Gnetum*."

The last paper was by Prof. C. Chilton, LL.D., F.L.S., on "*Deto*, a Subantarctic Genus of Terrestrial Crustacea."

No new species are described in the paper, but the six species are fully described and compared and their appendages figured.

June 18th, 1914.

Prof. E. B. Poulton, F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 4th June, 1914, were read and confirmed.

Mr. Francis Druce, M.A., and the Rev. Edward Francis Linton, M.A. (Oxon.), were admitted Fellows.

Mr. Walter Henry Charter, Mrs. Edith Jane Durrant, Mr. Linnæus Eden Hope, the Rev. John Robert Lewis Kingon, M.A., Mr. Robert Ray, B.Sc. (St. And. & Cape), and Miss Clotilde von Wyss, were elected Fellows.

The President announced that the Annual Dinner would take place on the 12th November next.

Prof. G. C. Bourne, F.R.S., Sec.L.S., briefly explained the scope of a paper by Mr. R. Douglas Laurie, M.A., "Reports on the Marine Biology of the Sudanese Red Sea.—On the Brachyura." (Communicated by Prof. W. A. Herdman, F.R.S., F.L.S.)

Mr. George Matthai, B.A., then gave his "A Revision of the recent Colonial Astræidæ possessing distinct Corallites" (communicated by Prof. J. Stanley Gardiner, F.R.S., F.L.S.), with explanatory drawings on the blackboard and a series of lantern-slides.

Prof Bourne contributed further observations, and the author replied.

Mr. Robert Paulson, F.L.S., exhibited two Lichens: *Lecanora isidioides*, Nyl., from the New Forest; and *Parmelia revoluta* var. *concentrica*, Cromb., from Seaford Downs, explaining their rarity and the difficulty of accounting for their geographical distribution in Britain; he further illustrated his subject by lantern-slides.

Mr. C. F. M. Swynnerton, F.L.S., gave the purport of his paper "Short Cuts to Nectaries by Blue Tits," referring to his

previous account of African ornithophilous flowers, read on the 5th March last, and showing photographs of injured shoots of *Ribes* on the screen.

A paper by the late Mr. William West, F.L.S., "Ecological Notes, chiefly Cryptogamic," was read in abstract by Dr. O. Stapf, F.R.S., Sec.L.S., who remarked that this paper was the outcome of a suggestion by Prof. Engler, F.M.L.S., that whilst abundance of observations existed of ecological facts regarding phanerogams, the cryptogams had been neglected. This paper was intended as the first of a series, which has been cut short by the unexpected and lamented death of the author, but who had sent in the summary read.

The following papers were, in the absence of their respective authors, read in title :—

Mr. R. J. Tillyard, M.A., F.E.S.—Life-Histories and Descriptions of Australian Æschninæ. With a Description of a new Form of *Telephebia* by Herbert Campion. (Communicated by Prof. W. A. Haswell, F.R.S., F.L.S.)

Miss Olga G. M. Payne, B.Sc.—On the Life-History and Structure of *Telephorus lituratus*. (Communicated by Dr. A. D. Imms, F.L.S.)

Mr. A. Grouvelle.—Cucujidæ, Cryptophagidæ, avec une description de la larve et de la nymphe de *Protominia convexiuscula*, Grouvelle. (Communiqué par M. le professeur J. Stanley Gardiner, F.R.S., F.L.S.)

Mr. Hugh Scott, M.A., F.L.S.—Mallophaga, Aphaniptera, and Diptera Puparia. (Percy Sladen Expedition.)

The General Secretary read an extract from a letter addressed to him by Dr. George Henderson, F.L.S., enclosing photographs of the fat-tailed sheep of the Punjab, which were also shown on the screen, one illustrating a small cart to support the tail of a sheep.

Dr. Stapf, referring to a remark in Dr. Henderson's letter, said he had seen similar sheep in Persia.

List of Contributors to the Sir Joseph Hooker Lecture Fund.

Anderson, Tempest, D.Sc.

Baker, J. G.
Barratt, W.
Berridge, Miss E. M.
Bickham, Spencer H.
Bower, Prof. F. O.
Bury, H.

Candolle, Dr. C. de.
Carson, Miss M.
Cockayne, L., Ph.D.
Cunningham, Col. D. D.

Darwin, Sir Francis.
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ABSTRACTS.

On the Origin of Species. By Dr. John P. Lotsy. (Communicated by Dr. Otto Stapf, F.R.S., Sec.L.S.)

[Read 19th February, 1914.]

Now that Mendelism has been at work for wellnigh fourteen years, it seems to me that the time has come to apply it to Darwinian Evolution. The theory for which I beg your attention is, it seems to me, but the logical consequence of Mendelism. I will, in the short time at my disposal, not be able to give more than a sketch of it. Yet in this short time I shall frequently have to dissent from the views of others. This, of course, does not in the least imply any disrespect for the persons from whose views I dissent. That I lay special stress on this is because I differ on some important points from the views of one whom I honour perhaps above any other, Charles Darwin, for whom I shall always have the greatest respect as a man, as an investigator, and as an exponent of deductions in strict accordance with the knowledge at his

disposal,—whose theory remains the starting-point for all time of a natural interpretation of the origin of species.

I will strive to be as little technical as possible. This will force me to use occasionally a sentence for what could be said in a single technical word, but I think it will make that I shall be more generally understood. To conclude this introduction, I beg of you not to spare my views in your remarks; by free criticism only we may hope to get somewhat nearer to the truth, which, I well know this, my theory at most can but partially unveil.

The first progress since Darwin was made by Alexis Jordan, when he showed that the Linnean species is not a unit, but a mixture of heterogeneous things in which the Jordanian micro-species are the most important. But even Jordan's species is a generalization, however small. To reach the root of the question, not the abstraction species, but the real individuals had to be studied. This fundamental insight, which Johannsen has called the principle of Vilmorin, is due to Louis Levèque de Vilmorin; it is the principle of isolation, which says that to study heredity one must begin to study the descendance of one single isolated individual. This principle has remained, yet our present views on heredity are not those of de Vilmorin. The reason is that de Vilmorin and all his contemporaries thought that individuals transmit their properties as an inseparable whole. Mendel has shown that this is not the case, that at the moment of reproduction the hereditary factors separate and get into a condition in which they can combine with other hereditary units. Unfortunately Mendel's publications long remained unnoticed. It is true that Darwin and especially de Vries had similar ideas, but these were conceptions, not proofs. We can therefore safely say that our modern ideas on heredity begin in that memorable year 1900, in which Mendel's law was rediscovered simultaneously by de Vries, Correns, and Tschermak. Since then every nation has contributed to Mendelian investigations, and England has taken, thanks to the work of Bateson and his school, a leading place in this international competition. In how far now do Jordan's conception of the micro-species, de Vilmorin's principle, and Mendel's law bear on the question of evolution?

Let us start from an inquiry into the properties of individuals. We know now that two classes of individuals exist, which I will designate as the pure and the hybrid class. In nature hybrid individuals are much more common than pure ones, each human being, for example, is a hybrid between his mother and his father. This fact accounts in no little matter for the difficulties which have beset the study of heredity. Darwin clearly perceived this, and began his studies by observing animals and plants under domestication, where pure individuals are much more frequent than in nature, because the breeder takes good care to isolate his breeding-stock, so as to prevent it from crossing.

In nature, pure individuals are relatively more frequent among

plants than among animals. This is due to the fact that many plants are hermaphrodites and self-fertilizers, while animals, as a rule, have to pair to raise a family.

To study heredity, one therefore does best to begin with the descendance of a self-fertilized plant. This Johannsen has done more carefully than anybody else. He has chosen some pure bean individuals and carefully compared the progeny of each parent with the latter. The result has been that the progeny differed from the parent, but only in such characters as are not transmittable. Apart from this modification, there was no difference between the descendants, so that selection among them had no effect whatever. Whether the small beans produced, or the largest ones were sown, the descendants had the same mean.

One might yet object that this failure of selection was due to the short time at its disposal. Fortunately we now know of a case to test this objection. Louis de Vilmorin started, in 1840, a living museum of different so-called varieties of wheat. He at once applied selection to their progeny, and this selection, of the most rigorous kind, has, uninterruptedly, been continued up to the present moment. Each year the best ear grown was selected, and the seeds of this ear alone were sown. Thus, among forms with branched ears, the best branched one, among such with long ears the longest one, etc., were selected. In 1911, the collection of original ears of 1840 was found at Verrières, and at once compared with the forms still cultivated there. It was shown that 36 of the original varieties were yet present. The best ear of each variety grown in 1911 was compared with the original ear of that variety, and it was shown that not one of these had changed their characters one little bit; so that more than half a century of selection among the descendants of a pure individual has no effect whatever *.

From all this but one conclusion seems to me possible:—

A pure individual, self-fertilized, has a descendance absolutely identical with it, as far as hereditary properties are concerned.

This fundamental fact I will call Johannsen's principle. We must now ask: Does every self-fertilized individual produce so uniform a descendance? We must answer: No; there are self-fertilized individuals which give a multiform descendance, namely, the hybrid individuals.

We must therefore conclude:—

Pure individuals have a uniform, hybrid individuals a multiform, descendance.

This seems strange. One might conclude from it that pure individuals transmit their properties as an inseparable whole, while hybrid individuals do not. Is there really so fundamental

* This statement was based on the communication by Mrs. C. and Dr. A. L. Hagedoorn, "Selection in pure Lines," Am. Breeders' Magazine, vol. iv. p. 165 *seq.* This communication, however, is erroneous, as Mr. Ph. de Vilmorin pointed out; one did *not* select the seeds from *one* plant, the best, but from several plants together, which were the most typical ones.

a difference? No; both behave similarly during reproduction. What, then, causes this difference? Mendel has given us the solution. By simple but well chosen hybridizations he has shown that no individual transmits its properties as an inseparable whole, but that each individual forms so many sorts of reproduction-cells as there are possible constant combination forms. Consequently, a pure self-fertilized individual forms but one kind of reproduction-cells, by whose combination but one kind of descendants, all identical with the parent, can arise, while a hybrid self-fertilized individual forms different kinds of reproduction-cells whose combinations cause the descendance to be a motley crowd.

As a hermaphrodite individual, of course, represents two parents in one, there is no reason why there should be a fundamental difference between the offspring of self-fertilized hermaphrodites and the offspring of a separate male and female. Indeed, there is not. If the male and female are pure and belong to the same type, their offspring is homogene; if they are hybrids, or because they belong to different types together form a hybrid, their offspring is heterogene.

We may therefore conclude: *The descendance of two pure individuals of the same hereditary type is as homogene as the descendance of a single self-fertilized pure individual, and absolutely identical with its parents as far as hereditary properties are concerned.* Or, in other words, pure individuals and their progeny are distinct, permanent, hereditary, and irreducible. But this is exactly what Jordan said already in 1873 of his micro-species.

In fact, the two conceptions are identical; but our present knowledge of heredity permits us a better definition of the small species, viz.:—

A species is the total of all individuals of the same hereditary composition, forming but one kind of reproduction-cells.

Or, in the language of Mendelism: *A species is the total of all homozygous individuals of the same genetic constitution.*

That, according to my view, is a species.

We must now try to answer the question, How do new species originate?

Conceivable are three different ways:—

(*a*) By acquisition of new characters and transmission of these to their progeny.
(*b*) By so-called spontaneous variation or mutation.
(*c*) By crossing.

The first two possibilities can be brought under the heading Variability. We will therefore ask:—

Does hereditary variability of any kind really exist?

We have seen already that variability does exist. The individuals composing a species are never alike, but only similar.

This kind of variability, which we now call modification however, does not count as it is not transmittable.

About the Inheritance of Acquired Characters

I will say but little. In the face of Johannsen's and Vilmorin's experience with selection in pure species, such an inheritance is very improbable. I should like to lay further stress on the fact that Vilmorin cultivates wheats from most varying parts of the world, and that some of them have properties which are frequently ascribed to the climate from which that particular variety came. Yet all remain perfectly true to type in Verrières. I have myself long believed in some kind of inheritance of acquired characters, in spite of the scanty evidence, because I saw no other way to account for the origin of new forms or of what is usually called Variability. Now I think that I *can* account for this in another way. I cannot myself offer any new facts to this vexed question, and will therefore refer to two authorities with whom I fully agree. On the botanical side Klebs, who has perhaps made more careful experiments bearing on this question than anybody else, states in Bateson's 'Problems of Genetics' in German what I here render in English:—"But up to the present no certain case is known in which the artificially produced character was transmitted through several generations under the usual 'normal' conditions."

On the zoological side Bateson says:—"In reading such papers as those of Semon or Kammerer, the thought uppermost in my mind is that to multiply illustrations of supposed transmission of acquired characters is of little use until some one example has been thoroughly investigated."

He then gives some examples of the kind of evidence he would consider conclusive and continues:—"Till evidence of this kind is published by at least two independent observers investigating similar material, I find it easier to believe that mistakes of observation or of interpretation have been made than that any genuine transmission of acquired characters has been witnessed."

This is exactly my position, and I know of no case furnishing definite proof of the inheritance of an acquired character. I know quite well that this is no way of treating the question, which is yet supported by so many able biologists, and I had intended to say more about it, but the time is too short.

Let us now consider

Spontaneous Variation,

or what is now more usually called mutation. Of this, two kinds more particularly concern us—the so-called progressive mutation caused by a factor being added to the stock of hereditary factors already present, and mutation by loss of a factor. The third and very doubtful kind of mutation caused by the change of a factor into another can be passed by, as the evidence for it is

altogether too scanty. Mutation by loss of a factor is of subsidiary importance to my theory; mutation by addition of a factor would be fatal to my conception that species are stable and reproduce themselves unchanged so long as no cross interferes.

Let us therefore consider first

Mutation by Addition of a Factor.

We all know that de Vries thinks he has good proof for the existence of this in the case of *Œnothera Lamarckiana*. There is no need to give a *résumé* of these universally known investigations. It suffices to remind you that *Œnothera Lamarckiana* self-fertilized throws plants unlike itself, the mutants of de Vries. Several of these breed true from their first appearance, others do not. This, of course, reminds one of the behaviour of a hybrid individual, although the proportions in which the aberrant individuals appear are such that we cannot bring this throwing in direct line with Mendelian segregation. Bateson and others, among whom myself, have therefore long ago suggested that *Œ. Lamarckiana* might be a hybrid. I yet think so, and am greatly strengthened in this opinion by the results obtained by Davis after crossing *Œnothera grandiflora* and *biennis*. It is true that he did not obtain a plant quite identical with *Lamarckiana*, but as he says "there is, I believe, no important character of taxonomic value presented by *Lamarckiana* .... that has not appeared in some of my hybrids. I have, as it were, surrounded the group of biotypes which we call the species *Lamarckiana* with a circle of hybrids that in various characters agree with the plants that have come down to us through the cultures of de Vries."

This, I think, is all that can be expected. To re-obtain a particular form from the heterogeneous progeny of a hybrid will always be a question of luck.

De Vries offers, in notes in his last book, some criticisms against Davis's view; one is that the *Œnothera grandiflora* used by Davis is no pure species, but a hybrid with *Œ. Traceyi*. This, of course, does not better the case for *Œ. Lamarckiana*; if true, it would be the result of two crosses instead of one. Another is that mutation is going on already in *Œ. biennis*, and thus is transferred to its hybrids with *grandiflora*. This objection would be of weight if it were proven that mutation occurred in the descendance of pure individuals of *Œ. biennis*. Such proof unfortunately does not exist *.

A third objection is based on circumstantial evidence. De Vries says that we know of no species hybrid behaving like *Œ. Lamarckiana*. This is true, but, probably owing only to lack of time to find and then investigate through many years such a hybrid, at least I am now in possession of one which might easily be mistaken for it.

It is the hybrid between *Nicotiana paniculata* and *N. rustica*.

* Neither is it furnished, to my way of thinking, by the recent publication of Stomps in the Ber. Deutsch. bot. Ges. xxxi. (1913) 166–172.

Both parents were cultivated by me in large numbers from seed given me by de Vilmorin. To test their stability I have grown large numbers without observing any than the usual modifications; anyhow, I am sure that neither throws giants nor dwarfs. The hybrid is hard to make, yet I obtained sufficient seed to raise a first generation of more than 1600 individuals. This was very lucky, because all, with the exception of a single plant, were absolutely sterile. The one fertile plant was this in a very imperfect degree, one branch of it only producing seed. From these seeds I could last year raise the second generation consisting of more than 2000 plants. At first this second generation looks so much like the former that I thought to have obtained a stable hybrid. As the plants grow older, however, and begin to flower and fruit, they show various combinations of characters derived from *N. paniculata* and *N. rustica*. Yet the segregation is decidedly hard to recognise, partly probably because there is not much to segregate, and because also, as the sterility of the first generation shows, combination is possible between a few only of the different kinds of reproduction-cells produced by the hybrid, partly because the characters themselves are hard to define. As a consequence of all this, one might really be excused if one mistook this second generation for a species with only fluctuating variability. In this respect, it would therefore resemble *Œ. Lamarckiana*, if this plant were a hybrid. The curious point now is that it resembles *Œ. Lamarckiana* in other respects, namely, it throws, like this plant, giants and dwarfs in what one might call mutational numbers, as the photographs which I now beg to throw on the screen will show. Of course, this is *not* a case like *Œ. Lamarckiana*; the giants here are due directly to the large stature of *Nicotiana rustica*, although they are even larger than this, but, if one did not know its hybrid origin, the case would certainly be quoted as one of mutation. All this, I think, tends to show that *Œ. Lamarckiana* is a hybrid, but as Heribert Nilsson in a very able paper has said: "After all, for the question of the existence of progressive mutation, it is not of the first importance whether *Œ. Lamarckiana* is a segregate from a specific cross; the crucial point is, Is it a pure genotype?

Which in my nomenclature says—Is it a pure species?

Now time fails to tell you of Nilsson's experiments; you will all have read them, and I think will agree with me that he has plainly shown that *Œ. Lamarckiana* is not a pure species, but a mixture of different types.

This makes *Œ. Lamarckiana* absolutely unfit to serve as such, as proof for the existence of progressive mutation, for nothing — no behaviour of chromosomes, however interesting this may be, and after Dr. Gates's careful investigations actually is—can let us make fall a particle of our claim that, to *prove* the existence of mutation, the purity of type from which the proof starts must be beyond the possibility of doubt.

With the proof that *Œ. Lamarckiana* is no pure species, the

experimental basis of the mutation-theory is lost, for de Vries expressly states:—

"I have in the course of the years cultivated many more than a hundred of such species, but essentially only one has in fact come up to my wishes."

This one was *Œ. Lamarckiana*.

I also know of no other case in which progressive mutation from a pure species has been shown to occur, so that I feel justified to say that so far there is no proof of the existence of progressive mutation.

Now about the question of

Mutation by Loss of Factors.

The existence of such mutations would not, as pointed out, be fatal to my theory. I even acknowledge the possibility of such a loss occurring accidentally, though I do not think it has been proved. As the question is of no vital importance, and as I have so little time, I will say very little about it.

At first sight, there seems to be plenty of evidence for this, but it nearly always is of a circumstantial kind. In this respect a *rôle* is played by the old belief in which we have been brought up, that of a certain number of forms resembling one another, one must be the original or at least be nearest to the original one from which the others arose. This leads to the involuntary conclusion that the most common form is the original and that the rarer ones are derived from it; so that, if we find a glabrous form among a large number of hairy ones, it is at once assumed that the glabrous one arose from the hairy one by loss of a factor. This, however, is a mere assumption. The aberrant form may just as well owe its peculiarity to a former cross.

A very nice case proves that this standpoint is well founded. Brainerd, the well-known investigator of American violets, describes a case in point. *Viola palmata* is a pubescent plant. Near Spring Valley, in the State of New York, a glabrous specimen referred to *Viola palmata* as a variety was found. Self-fertilized seeds of this specimen gathered after its transplantation in Brainerd's garden were sown, with the result that segregation was observed in plants with the deeply lobed leaves of *V. palmata* and such with the uncut leaves of *V. papilionacea*. The apparent *V. palmata*, var. *glabra*, was thus shown to owe its origin to a cross with *V. papilionacea*. Later on, Brainerd found at Tyron, North Carolina, very far from the first find in New York, therefore, another colony of a glabrous *Viola palmata*, which in the same way was proven to be a hybrid with the glabrous *V. papilionacea*.

This case alone, I think, shows that for the proof of the existence of mutation by loss of factors the purity of type from which we start is the very first requirement.

Hagedoorn thinks he knows of the existence of seven cases of

mutation by loss of a factor which fulfil this requirement, viz., two in the case of *Antirrhinum*, two in *Avena*, two in the mouse, and one in the case of the fly *Drosophila*. This number certainly is exceedingly small in comparison with the large number of cases reported, and even in these cases I am not yet convinced that they cannot be explained by an impurity of the material in respect to this factor. But, as I say, I acknowledge the possibility, and, as the question is not vital to my theory, I think what I say suffices in consideration of the short time at my disposal.

With the above restriction, I therefore come to the conclusion that species in the sense I define them are stable for an indefinite time, and that inheritable variation does not exist.

What then is the

Reason of the Apparent Variability of Species.

Although of a complicated kind, this reason can now be explained in a few words. In the first place, we now know from Jordan's work that a Linnean species is no unit, but contains a great many micro-species. Consequently, as Jordan points out, the Linnean species is a collection of specific forms which in future ought to constitute subgenera or genera in a new and more scientific classification.

As such, it can also, without objection in future, for convenience' sake be retained in systematics; and I see no objection to continue to speak of, say, *Antirrhinum majus*, if we only keep in mind that we mean by this an aggregate of micro-species and their hybrid derivatives.

For evolutionary questions, however, the Linnean species is of no use and its apparent variability a snare.

I quite agree with Bateson when he says:—

"When this variability is sorted out, and is seen to be in part a result of hybridisation, in part a consequence of the persistence of hybrids by parthenogenetic reproduction, a polymorphism due to the continued presence of individuals representing various combinations of Mendelian allelomorphs, partly also the transient effect of alteration in external circumstances, we see how cautious we must be in drawing inferences as to the indefiniteness of specific limits from a bare knowledge that intermediates exist."

It is, indeed, my firm conviction that, with the possible exception of mutation by loss of a factor, no transmissible variation exists.

One could yet make one objection against this view, based on the occurrence of

Bud Variation.

It was this point especially which led Darwin to decide against Pallas's idea that all variability was due to an original cross. My position is that bud-variation cannot serve as such, because there is no proof available that bud-variation ever occurs in a pure species; all cases reported seem to be explainable on the assumption that they took place in a hybrid, by segregation in

one of the somatic cells, by which division a line of descendants originates, lacking a Mendelian factor. This was carefully investigated in the potato by Evans, who concludes that all cases of bud-variation observed by him are caused by loss of a factor, which in normal condition mendelizes. Furthermore, Cramer, in his important study of the literature of bud-variation, a quarto of 474 pages, states that he is unaware of a single case of such a variation being due to the gain of a factor.

I have long thought that there was a case of bud-variation known in an undoubtedly pure line—namely, that described by Johannsen in his bean-cultures; but the discovery of Nilsson-Ehle that a supposed pure line can be pure in respect to one character and hybrid in respect to another, shows that this case also fails to furnish proof. Anyhow, even if bud-variation by *loss* of a factor occurred in a pure species, it would not be fatal to my theory. My standpoint, that with the possible exception of mutation through loss of a factor species are stable, now forces us to ask, How do new species arise? The answer is, I think, by

Crossing.

We all know that a cross between two so-called varieties gives a hybrid, which segregates in the next generation in a number of forms, of which some at least are stable and can have other properties than the varieties originally crossed. Consequently, there is abundant proof that so-called new varieties arise by crossing. Inasmuch as I have defined such stable varieties as micro-species, and as Jordan has shown that the Linnean species essentially is nothing but a collection of such micro-species and their hybrids, I could conclude without further ado that species originate by crossing, were it not that there is a general feeling among biologists that between a variety and a micro-species there yet is a difference. If this were only based on a feeling or on an assumption in regard to the possible behaviour of factors, we could let it pass as outside the possibility of proof or refutation, but this, thanks to de Vries, we cannot do. De Vries has claimed that it must be possible to judge, from its behaviour after crossing, whether a certain form is a species or variety. Species after this contention must give stable varieties segregating hybrids.

The difficulty of the test lies in the so frequent sterility of micro-species referred to different Linnean species, which should, if possible, be used for such a test. Happily, Prof. Baur in Berlin has found that many species (in the old sense) of *Antirrhinum* are fertile *inter se*. He gave me, very liberally, the seeds of some of these hybrids, which allowed me to study the progeny of the crosses *Antirrhinum glutinosum* by *A. majus* and *A. sempervirens* by *A. majus*. I have raised great numbers of these, up to 16,500 in one year, and think I am justified in concluding that, far from being stable, these hybrids, on the contrary, show an enormous segregation. If the time permits, I will demonstrate to you at the

end of my address a large number of lantern-slides and specimens showing this plainly: for the present, I must refer to the plates here present, and wish to call your attention especially to the plates with large flowers hanging in front of you, showing the very great alteration in the form of flowers which result from such a segregation.

From my experiences with *Antirrhinum*, I think I am justified in concluding that the difference claimed by de Vries for varieties and species does not exist. One might yet object that this segregation were induced by the impure condition in which *A. glutinosum* must necessarily be used, because it is self-sterile and every individual in consequence itself a hybrid. I do not think that this small degree of hybridity has any considerable effect on the enormous segregation going on, but fortunately we have proof in the case of *A. sempervirens*, which can be had pure, that segregation in this case also, after crossing with *A. majus*, takes place in the same manner.

It was further shown that sooner or later stable segregates arise, so that I think the proof is there that species can arise by crossing, and I personally think that new species always originate in consequence of a former cross.

Now, if my contention is right, that species are stable and not affected in their hereditary constitution in any way except by crossing, it must be possible to re-obtain, with the certainty of a chemical reaction, any desired form—provided it is not of too rare an occurrence among the segregates,—if we can only obtain the pure species from which this form was originally obtained.

I have tried to put this contention to the test!

Shortly after the introduction of *Petunia violacea*, about 1830, this species was crossed with *P. nyctaginiflora*, and soon after Petunias were obtained with green-rimmed petals. These have since disappeared, as has also the original *P. violacea*. It has been pointed out by Bailey and by Correns that the *P. violacea* of the trade, which was used by the latter in crossing, is *not* the original *P. violacea*. By a lucky chance, however, this original *P. violacea*, cultivated since its introduction, is still present in the cultures of Vilmorin. With it, kindly given to me by my friend, Philippe de Vilmorin, I have crossed *P. nyctaginiflora*, and among the segregates in F_2 obtained in large numbers the long-lost green-rimmed Petunias, drawings of which are here present.

P. violacea and *P. nyctaginiflora* have thus retained in Verrières their original constitution and produce now, as in 1830, green-rimmed Petunias with the certainty of a chemical reaction.

This certainly speaks strongly for the constancy of species, but I think here is a case which brings almost irrefutable evidence of this. It is the case of *Cytisus Adami*, which, as we all know, is a graft hybrid between *Cytisus purpureus* and *Cytisus Laburnum*. For more than a hundred years the cells of these two different species have lived in intimate connection, yet the branches arising "by reversion" furnish seeds from which both species arise in

absolutely pure form. No better experiment could, to my mind, be made to prove the constancy of species than this involuntary one.

Now, after all this evidence for the constancy of species and their origin by crossing, the question remains:

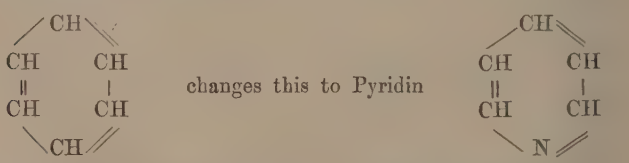
What is the Vera Causa *of the Change produced by Crossing?*

I think we can safely say that it is the introduction of a new factor, and that by this the problem of the origin of species is brought into line with the problem of the origin of chemical compounds. In species it is the new factor, in chemical compounds the new element introduced, which causes the change.

Just as the introduction of a new element may cause a great or small effect, dependent on the kinds of elements already present and the kind of element introduced, the new factor may cause a small or a great change.

Not the size of the jump is essential; the essential thing is that the introduction of the new factor causes a definite change, so that no intermediate stages between two species different in one factor occur. From this it might seem that my theory requires as many links as Darwin's does. Yet I do not think so. Considerable jumps *and* small jumps are possible, according to my views. There are among organisms differences of a major and differences of a minor degree. So, for example, all vertebrates are, as far as their skeleton is concerned, built very much on the same plan. So are Angiosperms in respect to their flower structure. Now such ground-plans, I take it, are caused by the introduction of a major genetic factor, and lesser changes in the ground-plan, like the transformation of a foot to the fin of a whale, being due to minor genetic factors.

I may illustrate this by a chemical analogy. The introduction of Nitrogen in the Benzol ring



and from this Pyridin all genuine Alkaloids are derived. So the introduction of the unknown factor x may have caused in some invertebrate the formation of the skeleton, and, if this be true, it is hopeless to look for a transitional form between an invertebrate and a vertebrate, as none such ever existed. The vertebrates then arose by a cross of two invertebrates, who together possessed the set of factors necessary for the formation of a skeleton, while each of them alone lacked one or more factors of it.

This brings me to the first origin of species. I think living organisms of different kind, *e.g.*, possessing different sets of

hereditary factors, have originated by some process of what one calls spontaneous generation. Progress was caused by different factors coming together after fusing, *e.g.*, after crossing of these Urorganisms.

The objection sometimes heard, that the shuffling of a pack of cards does not cause new cards to appear, therefore, does not fit the case. I take it, a large number of incomplete packs of cards originated, so that every time two of them were shuffled there *was* an addition of new cards. Of course, this part is entirely hypothetical.

Now let us consider some

Possible Objections.

The first one will probably be that the one case of *Antirrhinum* does not prove that all species hybrids segregate. Of course, it does not, but the number of cases showing segregation daily increases. I know already of the following ones:—

Primula grandiflora × *officinalis*.	Naudin.
Petunia nyctaginiflora × *violacea*.	
Linaria purpurea × *vulgaris*.	
Different species of *Dianthus*.	Baur.
„ „ *Aquilegia*.	
„ „ *Melandrium*.	
Pisum sativum × *quadratum*.	Sutton.
„ *arvense* × *quadratum*.	
Mirabilis Jalappa × *longiflora*.	Correns.
Lappa officinalis × *tomentosa*.	Heribert Nilsson.
Different species of *Rubus*.	Lidforss.
Argemone mexicana × *platyceras*.	Philippe de Vilmorin.
Digitalis purpurea × *grandiflora*.	
Eremurus Bungei × *Olgæ*.	
Impatiens Petersiana × *Holstei*.	
Triticum × *Secale*.	Jesenko.
„ × *Ægilops*.	Louis de Vilmorin.

The two latter cases are even so-called bigeneric hybrids.

We further know of micro-species belonging to the same Linnean species in which, after crossing, segregation occurs; so in the case of the micro-species of *Erophila* crossed by Rosen, which segregate in F_2, but then, if no mistake due to small numbers has taken place, become stable, probably owing to apogamy. As this case therefore is complicated by something not quite elucidated, I can here, by the kindness of Mr. Kristofferson of Lund, communicate that micro-species of *Viola arvensis* crossed among themselves and crossed with similar forms of *Viola tricolor*, segregate in the most approved manner.

It therefore seems that segregation occurs generally among so-called species hybrids as well as among so-called varieties, which was to be expected, if there is no difference between species and variety.

Of course, one can yet offer as an objection the *Œnothera*-crosses of de Vries. Now, I think that as long as we do not know what kind of things these forms crossed really are, their

behaviour cannot profitably be discussed. Furthermore, I do not think that the analysis has been carried far enough in any case to say whether segregation takes place or not. I do know that in cases where de Vries saw only two forms in F_1 and F_2, I saw several at least in F_2, without, however, being able to fix their limits. This difficulty is due to the fact that the differences between these yellow-flowered Œnotheras are, in their visible effects, more of degree than of kind; so that a Mendelian distribution of the characters over the offspring would be very hard to recognise. The large number of factors involved, the difficulty of sharply limiting the visible characters caused by them, the possibilities of coupling and repulsion, all tend to form an F_2 which cannot be entangled, and which one can, by proceeding arbitrarily, bring to one, two, or several forms. Under these circumstances, I cannot attribute much weight to the apparent exception de Vries's crosses furnish *.

It may yet be objected that other first-rate observers have described stable species-hybrids. A very interesting case of this has recently come to my knowledge. Darwin got from a friend hybrid-geese from the cross of the common goose with a Chinese goose. From them he obtained five birds of the second generation, which he says, "in every detail resembled their hybrid parents."

I will try to repeat the experiment, but think it very likely that the apparent uniformity is due to the small number of segregates obtained. In this respect it is very interesting to know that Mr. Heribert Nilsson, who has allowed me to communicate this here, has succeeded in showing, for some, at least, of Wichura's stable *Salix*-hybrids, that these are not stable, but segregate very considerably. I have further details of these experiments, which I will be glad to show anyone present interested in them.

Another objection which is offered again and again is that a difference exists between species and varieties, inasmuch as the first are mostly sterile after a cross, while the latter are not.

Now, if my contention is right, that only micro-species exist, this statement can be transcribed to this sentence, that micro-species different in many factors (as such referred to different Linnean species are likely to be) will less easily cross than such which differ in but few factors. Now I would see nothing unexpected in this, and nothing which would tell against my theory—no more, in fact, than the objection that certain chemical substances show affinity and others do not, tells against the theory that chemical compounds are formed by the union of other chemical substances already present. But *is* the contention quite true? Are species essentially so much more sterile *inter se* than varieties? By essentially I mean if we take into consideration secondary difficulties against crossing, as differences in size between the animals which have to copulate, poisonous substances on the stigmata of flowers, etc. We must not forget that we have cases where reproduction-cells so closely related as the pollen-

* Renner has since shown that de Vries's results can be explained in a Mendelian way: see Flora, cvii. (1914) 115–150.

cells and ovules of a hermaphrodite plant do not succeed in union, as the many self-sterile plants show. Furthermore, I, till now, could not get seeds by crossing several of my different forms of self-sterile *Antirrhinum glutinosum*, while I can by crossing others.

In all cases of apparent sterility, one must not conclude too soon as to sterility really being absolute. I have for several years tried in vain to make the cross *Primula acaulis* × *elatior*, but am now in the possession of the first generation. I think that in this case the question is, that certain micro-species of *P. acaulis* are fertile with certain micro-species of *P. elatior*, while others are not. There are more curious cases yet. Miss Digby has shown that *Primula floribunda* and *P. verticillata* give the hybrid *P. kewensis*, which produced only thrum flowers and was sterile. It was multiplied by cuttings for about five years, when a single pin-flower appeared on one individual. This pollinated from a thrum flower gave seeds which sprouted! If we consider, on the other hand, that we now know already a considerable number of fertile species-hybrids—yes, even some fertile bigeneric hybrids—I doubt whether the statement that species generally are infertile after crossing, while varieties are fertile, is so true as it seems, if we exclude from secondary difficulties. I believe we must know much more of the real causes of sterility before this statement can be fully accepted.

Yet it would be very interesting to obtain experimentally from the cross of two self-fertile forms such which were sterile *inter se*. Now Rosen *has* already obtained something of the kind. The two closely similar micro-species *Erophila stricta* and *E. elata* can be crossed, but the offspring is nearly completely sterile. As in the light of Rosen's experiments *E. stricta* and *E. elata* are themselves most probably segregates of a cross, and themselves are quite fertile, we have a near approach to the desired proof. Perhaps a case among my segregates from the cross *Antirrhinum glutinosum* and *A. majus* is better still, yet the evidence is, as the experiment was but once made, so far not perfect. *A. glutinosum* is self-sterile, *A. majus* self-fertile. The first generation is self-fertile, in the next segregation both self-fertile and self-sterile forms occur. Now I crossed in the second generation, for another purpose, a self-fertile zygomorphous plant with the pollen of a self-fertile peloric plant. This was done with ten flowers, and, as I did not doubt the result and other work needed my attention, I did not look at the plant until the time for gathering the fruit came. To my great surprise none of the ten flowers had set fruit, while nearly all other flowers had set beautifully by self-fertilization. It seems rather difficult to think that this was mere accident, yet of course, the experiment will be repeated.

One might yet object that crossing is not of sufficient frequency in Nature to allow its playing a considerable *rôle* in the formation of species. I think it is fully. According to Kerner, no less than 1000 wild-growing hybrids have been found in the 50 years

preceding his writing, and then much less attention was paid to these questions than now. Recently Brainerd, for example, has described no less than 75 certain hybrids in a single tribe of *Viola* alone.

After thus having given my objections, I may be allowed to call your attention to some of the

Advantages.

In the first place, the circumstance, that a group of new species arises simultaneously, and *not* one species *from* the other, simplifies matters, inasmuch as we need not look for intermediates between all these species—they never were chains. Of course, I do not mean that no species in the group have been exterminated, but they were no *transition* forms. This explosion-like appearance, furthermore, seems to me much more in accordance with what we find in Palæontology than a very slow transformation from one species to another. The point urged, that the introduction of a major factor can cause considerable change, explains why we find no transitions between great phyla, and makes their great antiquity better understood. So it is quite easy to see how Angiosperms can—as geological evidence, so far as it goes, shows us—have appeared very quickly in very different forms. The great diversity of new species which can arise from one cross makes it, furthermore, simpler to comprehend how, without any slow process of adaptation, forms can arise fitted to live under the circumstances in which they are born. They arise ready-made, survive if they fit, die if they do not. Another advantage to my mind is that it opens the possibility to regard the origin of species in the same way as at the origin of chemical compounds, and thus brings further evidence for the assumption that similar laws reign over lifeless and living Nature. In this respect, I want to lay special stress on the fact that I do not, as de Vries did, consider the factors or gens as living particles, but as chemical substances not living themselves, but the result of whose interaction is what we call life. In this respect, as in many others, I perfectly agree with Hagedoorn. I think that recent investigations tend to support this view. In the laboratory of Kamerlingh Onnes in Leiden, Becquerel has kept immersed in liquid air during six weeks small glass tubes containing, in a vacuum as high as could be obtained, decorticated seeds of *Medicago sativa* and of mustard, and spores of different kinds of mould and bacteria. After this these same tubes were immersed in liquid hydrogen for 77 hours. The seeds, moreover, were kept during a year and a half in the vacuum-tubes, the spores and bacteria even two years. Yet they sprouted!

In these experiments, which united for the first time an extreme desiccation, an extreme lack of air, and very low temperature, even down to 253° below zero centigrade, the protoplasm, deprived of water and oxygen, and under hardly any atmospheric pressure, must have lost its colloidal stage, and cannot have shown a trace

of physical or chemical phenomena of life. Yet these grains and spores sprouted and developed to normal plants. I see no way of explaining these facts than by the assumption that life is the result of forces working between substances not living themselves, and this is in perfect accordance with the view that the genes need not be living particles, but may be, as Hagedoorn suggested, autocatalytic ferments.

I can now state in a very few words the gist of my theory: -

New species are born into the world as a result of a cross between already existing ones. The species born is ready (finished) and stable, not subject to any hereditable variability, except, possibly, occasional loss of a factor. Nature, consequently, "facit saltum." The jump, however, may be very small; the size of the jump is not essential, the essential thing is that there are no intermediate stages between species.

There can be no question of the making of a species by any form of selection by which certain individuals belonging to the species to be transmuted are favoured above others, because the bestowal of such favour could have no effect, as all individuals belonging to one species are identical so far as inheritable qualities are concerned.

Or, in very few words, intraspecific selection is impossible.

Interspecific selection remains possible. Whether it occurs or not, depends on the fact whether extermination goes on indiscriminately or whether in all—or, at least, in the majority of cases—the best species are selected.

This problem is a very vast one; without a minute knowledge of the struggle for life in a particular case it seems to me unanswerable. Anyhow, it lies outside the pale of my subject: the *origin* of species.

A discussion followed:—

Prof. W. Bateson said he was glad that Professor Lotsy had brought his theory for discussion to a meeting of the Linnean Society, the classic ground for such a debate. The paper should lead naturalists to appreciate the difficulties besetting both the theory of evolution and the practice of the systematists. With Professor Lotsy's main contention he had great sympathy. On present knowledge the homozygous forms were the only definite entities. It was true that most of the facts we had formerly taken as evidence of variability must now be otherwise interpreted. He supposed that all who had witnessed the polymorphism of a large F_2 family, raised from an original cross between two very distinct parents, must have wondered, with Professor Lotsy, whether it may not have been by such crossing that the origin of species has come about. Prof. Bateson admitted that crossing was the most probable origin of very many species. He held, nevertheless, that variation by *loss* of factors was clearly proved to be a real phenomenon of genetic physiology. Whether any factor could be *added* was doubtful. Certainly, at present, there was no

proof of such an occurrence; for, logically at least, it was always possible to invert a factorial expression and to represent a supposed addition as the loss of an inhibiting factor.

Prof. Bateson was followed by Prof. J. B. Farmer and Mr. P. L. de Vilmorin; then Dr. Gates said:—

Let me say, in the first place, that I heartily agree with Dr. Lotsy that species may and do frequently originate through crossing. There is abundant evidence of this in *Œnothera* and in various other cases, evidence which Mendelians have, if I understand them rightly, been inclined until quite recently to deny. The question at issue is, then, whether this is the only method by which new forms arise.

I believe we will also agree with Dr. Lotsy that the individual, and not the species, is the real organic unit. Had Dr. Lotsy adhered to this thesis, I might, perhaps, have been able to agree with him to the end. But he immediately discards this idea, and adopts the conception that the genotype of Johannsen is a constant, immovable, and eternal unit. The genotype or pure line is, however, only the offspring of a relatively pure individual. I have grown scores of such races in *Œnothera*, and they differ from each other in every conceivable manner and to every conceivable degree. By selfing individuals in successive generations innumerable genotypes or (as I prefer to call them) biotypes may be obtained, which differ from each other in smaller and smaller details. The biotype is, therefore, merely a concept, like the species, only varying within narrower limits.

The work of Jennings with *Paramecium* has shown that pure lines may be isolated by selection, differing from each other constantly by fractions of a micron in average length. He specifically points out, however, that these differences are only relatively constant, and that by continued selection within a pure line races may be obtained which differ from each other less and less, down to the limits of accurate measurement. Continued selection and pure-line breeding within a species or a pure line multiplies the number of races and continually decreases their variability, unless and until (and this is a fundamental point of difference between Dr. Lotsy and myself) a germinal change occurs.

We may then inquire, what is the justification for Dr. Lotsy's dogmatic assertion that there is no such thing as a germinal change? In this sweeping denial he far outdoes the Mendelians themselves, and succeeds in reducing Mendelism as a philosophy of evolution to a *reductio ad absurdum*. We have been accustomed to being asked by the Mendelian School to believe that all evolution "from *Amœba* to Man," or in any other direction you may choose to consider it, has taken place by the "loss of unit factors," or, as I would prefer to say, by negative mutations. Thus, if a race of guinea-pigs is obtained, having six toes instead of five, we are asked to believe that this race has not suffered a germinal change, far less has it gained anything. No, it has

merely lost an inhibitor for six toes, which all its unfortunate ancestors all the way back to *Amœba* possessed, but could not get rid of!

Any such view of evolution founded only upon the cases of negative mutations is sufficiently bizarre, but Dr. Lotsy would deny us even this! However, I think we may not only agree with the Mendelians that negative mutations through the loss of unit-characters do frequently occur; but we can, moreover, confidently assert that mutations of a divergent and positive kind occur as well.

Referring now to the Œnotheras, my view has been that the species *Œ. Lamarckiana* is in a condition of germinal instability, leading to various types of germinal change in certain of the offspring. The cause of this condition, whether from crossing, change of climate, or other agencies, is immaterial to the present argument. The nature of some of these changes has been clearly demonstrated by the cytological work of others and myself; and I challenge Dr. Lotsy to offer an explanation of the origin of *Œ.* mut. *lata* and *Œ.* mut. *gigas* on the basis of this theory, which is at the same time in harmony with the cytological facts. I should be glad if Professor Bateson would also answer this question in terms of factors.

Dr. Lotsy says the fundamental point is whether the mutants "owe their origin to the addition of a new factor to *Œ. Lamarckiana*." But I deny that the fundamental point is anything of the kind. The crucial point is whether an inherited germinal change, *i. e.* a mutation, has taken place. The behaviour of the chromosomes shows with certainty the nature of the visible change which has occurred in the two cases I have cited. In *gigas* the chromosomes have changed from 14 to 28. In *lata* they have changed from 14 to 15. I challenge Dr. Lotsy to produce an explanation on his theory which will show that the same kind of change occurred in both these mutations.

Six years ago I was able to show that during the reduction divisions in *Œnothera*, both members of one pair of chromosomes occasionally go into the same germ nucleus. This is how *lata* originates, and Miss Nesta Thomas and myself have recently shown that *lata*, no matter from what source, invariably has 15 chromosomes. *Lata* does not therefore possess a new unit-factor, but a duplicate for one of its pairs of chromosomes, and the essential germinal change or mutation occurred in the reduction division which resulted in a germ-cell having 8 chromosomes. This process is directly contrasted with the regular Mendelian recombinations of characters. If this does not satisfy Dr. Lotsy as a germinal change, then nothing but a special creation of a new unit-character would satisfy him. I have found the same cytological change occurring in the wild *Œ. biennis* in America, which is regularly self-pollinating.

Again, in *Œ. gigas* we have an experimentally demonstrated case of the sudden origin of tetraploidy, *i. e.*, the $4x$ number of

chromosomes. This is a well-known phenomenon among wild species, and 30 or 40 such cases are now known. There is every reason to suppose that tetraploidy has originated in these cases as it has in *Œ. gigas*. In this mutant, wherever the doubling of chromosomes occurred in the life-cycle, it obviously took place suddenly in a single cell. Whether this was before or after fertilization is a detail, but the accompanying increase in the size of the cells and nuclei alone accounts for most of the peculiarities of *gigas*, even such as the short fruits and the increased susceptibility to frost.

Instead of this view, based on the cytological facts, Dr. Lotsy offers a theory of purely hypothetical genes, which no one has ever seen and which may not exist at all. He begins by assuming that *Œ. Lamarckiana* is a heterozygote in the Mendelian sense. This is a pure assumption, and the numerous crossing experiments of de Vries show clearly that his race was not heterozygous in the Mendelian sense. Is it not obvious that the cytological facts furnish a useful check on unbridled speculation about hypothetical genes?

There are many other criticisms which might be made, but I will only recall to your minds the fact that there are many well-authenticated instances of mutation in the Bacteria. If mutations can occur in bacteria in the absence of crossing, why not in higher plants?

Dr. Keeble urged, in opposition to the views expressed by the previous speaker (Dr. Gates), that the members of lines of plants which are homozygous for a considerable number of Mendelian factors are remarkably uniform and stable. For example, in his experiments with peas (*Pisum sativum*) he had bred races which grew at the same rate, reached equal heights, flowered at the same node and at the same time. Hence, it may be conceded that pure lines are very stable, although the evidence available at present does not allow of the claim that no variation occurs among the members of a pure line. The cytological evidence brought forward by Dr. Gates may conceivably be read in either way—for example, a giant form may be gigantic because it has double the number of chromosomes possessed by the ordinary form from which the giant arose, or it may have the double number of chromosomes because it is a giant.

Professor Dendy expressed the opinion that the development of organisms, whether phylogenetic or ontogenetic, was a dual process. There was, first, what might be called the fundamental development, which might be compared to the main theme of a piece of music; and, second, a series of features superposed upon the fundamental development, which might be compared to variations on the main theme, and which were due to crossing or hybridization. He asked his hearers to try to work out for

themselves what might have happened if sexual differentiation had never arisen and the complications introduced thereby had never occurred. He believed that divergent evolution would still have taken place in a tree-like manner under the influence of changing environment, and that species would have become separated from one another by the dying out of connecting links, as Charles Darwin supposed. He called attention to the case of isolated species which had undergone evolution on oceanic islands, such as the flightless Dodo of Mauritius and the Solitaire of Rodriguez. Such species were generally believed to have originated from ancestors which had reached their respective islands by flight and which must have been very different from their descendants. It was hardly possible that there had been any opportunity in such cases for crossing between distinct varieties or species, and yet it would be absurd to say that the flightless birds, which had gradually lost the power of flight as the result of changed habits in their new environment, belonged to the same species as the ancestral forms which first reached the islands. He thought that the innumerable more or less clearly recognizable forms that could undoubtedly be produced by hybridization played but a small part in evolution, and that it did not very much matter whether they were termed species or not.

Sir Francis Darwin. I should be glad to be allowed to say a few words, since I have a hereditary interest in any evolutionary discussion held in this room.

Those who know Dr. Lotsy or his writings will not need to be told that his paper is in no sense hostile either to Darwinism or to the author of the Darwinian theory. I should like to thank him for his attitude in regard to my father.

As far as I can judge, Dr. Lotsy has made a fair and striking statement of his theory; but I have not the detailed knowledge of genetics needed to form a critical judgment of his views.

A point that interests me especially is the bearing of Dr. Lotsy's theory on the adaptation of flowers to insect visitors.

Variation is to a species what movement is to an individual, since without change of form, temporary or permanent, no response can be made to new conditions. The life of a species must depend on appropriate variation as much as that of an individual on movements. If Lotsy is right, and variation depends on crossing, there is obviously a powerful selective force at work on such floral structures as favour cross-fertilization. This is a subject which Dr. Lotsy has referred to in the proofs of his French paper, which he was good enough to show me.

Dr. A. B. Rendle pointed out that the sudden appearance of a large number of new species as the result of segregation following a cross suggested the origin of genera rather than of species.

He did not think that the assumed stability of the genotype or microspecies was borne out by the results of close critical study which often revealed micro-microspecies sometimes unrecognizable except to the highly trained eye of the expert.

Even granting Dr. Lotsy's hypothesis as one possible means of origin of species, representing new combinations of previous characters as the result of crossing, this seemed inadequate by itself to account for the enormous variety of species all more or less closely adapted to their environment.

Referring to the remarkable series of Antirrhinums shown by Dr. Lotsy as the result of segregation after crossing, the speaker did not consider these comparable with natural species. A large proportion were of the nature of monstrosities, and departures from the normal in floral structure (even though very slight) would militate against pollination of the flower by insect-agency and thus tend towards elimination of the form. It was important to carry out the experiment of determining what would happen to such a series when left to itself in nature.

Professor MacBride, F.R.S. (*visitor*), said that the theory so ably explained by Dr. Lotsy was, in effect, exactly the same as Weismann's famous theory of heredity based on his theory of ids and determinants, and the supposed different collocations of these effected by the process of fertilization.

In the logical development of his theory Weismann had to account for the differences between his ids or hereditary units, and to do this he had to postulate the inheritance of acquired characters in the unicellular ancestors of the higher plants and animals, and thus to give away in principle his whole case.

Lotsy's view fails to account for the phenomena of recapitulation in the development of animals. If we take the life-history of the hermit-crab, we find that the larva has a symmetrical abdomen, but that when the larva gives up its free-swimming life and drops to the bottom it inserts its abdomen into an empty univalve shell, and the abdomen becomes curved accordingly. Now, if the larva be prevented from obtaining a shell its abdomen becomes curved, but not so much as when a shell is available. Is it seriously suggested that the "factors" for a curved abdomen were brought together by some accidental sexual union, and that these factors became operative at the exact moment when the larva dropped to the bottom?

Evidence is gradually accumulating that the environment can and does affect heredity. Kammerer's work on the inheritance of skin-colour in the Salamander and Agar's work on the shape of the carapace in Daphnids both point in this direction.

If the results obtained by these experiments are criticized as wanting in clearness compared with the results obtained by Mendelian crossings, it may be answered that experiments on the effect of the environment are much more difficult to carry

out than ordinary Mendelian experiments, and require an immensely longer time in order to obtain a definite result.

[Dr. Lotsy talks airily of the advantage which his theory enjoys of enabling us to dispense with intermediate links connecting distinct phyla, and even suggests that the vertebrate skeleton may have sprung into existence at once like Minerva from the head of Jove, owing to the bringing together of several factors by sexual union. In answer to this, it may be stated that in those cases where the palæontological record is at all well preserved, as it is in the case of the evolution of the bony elements of the vertebrate skeleton, there is no evidence of the sudden appearance of new characters, but increasing evidence of evolution by slow and gradual change.]

Professor F. E. Weiss, stated that, while agreeing with Dr. Lotsy that in some genera, like *Rosa* and *Rubus*, which have a very large number of species, some of these may have resulted from crossing; yet in other cases where hybridization takes place no great variety of forms has resulted under natural conditions. He continued:—

I have been engaged for some years in an investigation of the hybrid between *Geum urbanum* and *G. rivale*, which is not infrequently found in Britain, and is often included in our Floras as *Geum intermedium*. I can fully corroborate Dr. Lotsy's observations as regards the segregation of species-hybrids in the F_2 generation. As already recorded by me at the Dundee Meeting of the British Association, the F_2 generation of *Geum urbanum* $\times$ *G. rivale* is very varied, some of the forms being quite distinct from either of the parents. Yet in nature these forms are very rarely met with, though the first hybrid is comparatively common and very fertile. The comparative absence of the multifarious segregates of *Geum intermedium* (so-called) must, I think, be due to the fact that it is more frequently fertilized by pollen from one or other of the parental forms—the result being, as I have found by experiment, a form resembling the parental species more closely than the hybrid. It is probable that in nature, by such crossing with the parent forms, the segregates of the hybrid tend to disappear. This must, I think, be commonly the case with hybrids in the case of genera adapted to cross-pollination.

With regard to the restriction of the term species for genotypic forms, it seems to me, we should never be certain whether plants, morphologically identical, belonged to the same species. Physiologically they might be diverse. This would, for instance, be the case with *Cardamine pratensis* and other self-sterile plants, where individuals only are genotypes, and consequently all the offspring are physiologically heterozygotes. Dr. Lotsy recognized this difficulty in speaking of *Linaria*.

Major C. C. Hurst said: We are much obliged to Dr. Lotsy for bringing forward this interesting question in such an able

manner. Some fifteen years ago, in 'Nature,' I ventured to express a similar idea based on studies of orchid hybrids. Since then the results of Mendelian experiments, as Dr. Lotsy has shown, have contributed largely to the facts on which the theory is based, and it may interest Dr. Lotsy to know that unpublished data of my *Berberis* hybrids provide another illustration of the possibilities of the "origin of species by crossing" perhaps even more striking than the remarkable series of *Antirrhinum* hybrids that he has exhibited here to-night.

At the same time I cannot but feel that the new facts have added some difficulties to our acceptance of the theory as a final solution of the problem of species. One difficulty is that, while crossing undoubtedly causes variation in the second generation, yet *in all plants that are naturally cross-fertilized* the new forms that arise must be for the most part permanently heterozygous and consequently unstable.

Another important question seems to arise out of our experiments. Is it certain that the morphological characters which characterize systematic species are of paramount survival value? Is it not possible that certain physiological characters, though less obvious, may be more important? May not such qualities as vigour of constitution and high productiveness be the critical characters which really determine the survival and distribution of organisms, whatever their morphological characters may be?

If so, the question of the "origin of species" fundamentally would appear to be a physiological rather than a morphological problem.

Experiments with cultivated plants and domesticated animals show that, except in some extreme cases, crossing certainly promotes vigour of constitution and productiveness. It may well be that in this respect, as in others, crossing is a most important factor in the evolution of species.

Mr. Arthur Sutton exhibited a lantern-slide showing a reproduction of Gerard's Wild Beet (*Beta maritima*), which is generally accepted as the source from which the

(*a*) Mangel Wurzel,
(*b*) Garden Beet,
(*c*) Sugar Beet,
(*d*) Spinach Beet,
(*e*) Sea-Kale Beet, or Swiss Chard,

have all been derived.

All of these perfectly distinct plants have been developed as sources of food supply either by root development, as in the case of Garden Beet, Sugar Beet, and Mangel Wurzel, or by development of foliage, as in the case of Spinach Beet and Sea-Kale Beet, and

will reproduce themselves absolutely pure if steps are taken to prevent hybridization. According to the author of the paper (Dr. Lotsy), we should have to consider that all these various and distinct forms have suddenly originated by the crossing of two individual plants of *Beta maritima*, rather than by long continued selection, as has generally been supposed. Beyond all doubt, selection has played a very important part in bringing these several forms to their present state of perfection.

I imagine we all felt that the paper afforded much valued light upon a very obscure problem, but that the Author's theory and claims by no means met all the difficulties of the case.

Miss E. R. Saunders stated that with much of what Dr. Lotsy had said one felt to be in agreement, but that when he pushed his theory to its extreme limit, when cross-fertilization was put forward as the *sole* cause of evolution, one felt unable to agree with him.

Certain cases had already been mentioned by previous speakers in which it was difficult to accept Dr. Lotsy's view. Many others might be brought forward, as, *e. g.*, the case of certain monstrosities which breed true, or the case of doubling in such a form as the stock. That the form of *Digitalis purpurea*, which is found to turn up from time to time, in which stamens take the place of petals, should be due to crossing, seems much less likely than that it should be due to some irregularity at some point in the process of cell-division, and the same with the Stock.

One cannot help feeling that Dr. Lotsy's theory does not take us back to the beginning, that it does not give us the opening chapter in the story of Evolution, and that it requires, to use his own simile, that the packs of cards should all have been incomplete to start with.

The President said that, at this late hour, he would only contribute a few words to the discussion.

The different species of animals do not freely interbreed, as many plants appear to do. However difficult it may be for the human zoologist to determine what is a species, the problem appears to present no difficulty whatever to animals themselves. It is exceedingly rare for these fundamental instincts to go astray, and, when they do, it is exceedingly rare for fertile offspring to be produced. Animals do not act upon the lax principles which, as botanists tell us, are freely followed by plants.

He also desired to associate himself with the arguments brought forward by other zoologists. A small portion of a species was often cut off from interbreeding with the rest by isolation on an Oceanic Island. According to Dr. Lotsy's hypothesis, the variation of such isolated sections should be checked—as a matter of fact, it was known in many conspicuous examples to be increased.

The study of the geographical races of animals pointed to the same conclusion. When two of these races met and interbred, as happened in Africa—where eastern and western forms came together at the Victoria Nyanza or the Great Rift Valley,—we ought to find, according to Dr. Lotsy's hypothesis, variation radiating in many directions. What we do find is either a series of variations intermediate between each of the two intermingling geographical races or else an abrupt replacement of the one race by the other. In the latter examples, a Mendelian interpretation was probable.

Dr. Lotsy:—The divergence of opinion expressed shows how little we really *know* of evolution. We can *imagine* several ways in which species can originate, but I think one way only is really *known* to us, viz. by crossing. I have therefore tried to show that a theory based on this fact is able to give a satisfactory explanation of species-formation in general. Of course it does not follow that this explanation is the right one, but I beg to call your attention to the fact that it is in agreement with two fundamental facts discovered by Jordan, viz., the constancy of Jordanian species, and the fact that the Linnean species is a kind of genus composed of vast groups of Jordanian species growing socially in nature. In this respect it is very satisfactory to me that Dr. Rendle thinks that the result of segregation following a cross suggests the origin of genera rather than of species.

The hour is too late for a detailed reply. I should like, however, to point out that I have limited my remarks to the *origin* of species, and not treated the question what becomes of these in the struggle for existence, etc. It seems to me that we must keep these two important questions more strictly apart than some of the speakers have done. I shall be glad to try to meet Dr. Gates's challenges as soon as he obtains similar cytological results by investigating an undoubtedly pure species, as he has obtained now on material of very doubtful, probably of heterozygous nature. Mr. Sutton's criticism has already been met above; *Beta maritima* is in nature already a mixture of several microspecies. To finish, I would beg all who criticise my views to keep in mind that the Linnean species is not a unit, but a mixture of heterogeneous things, in which the Jordanian species are the most important.

Doubtless opportunity will offer itself to answer the objections made, more fully in some future paper.

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Carved rhinoceros horn from Lady Smith, formerly in the possession of Carl von Linné.

1874.

Subscription portrait of George Bentham, by Lowes Dickinson.

George Bentham, Esq., for expenditure on Library, £50.

1875.

Legacy from James Yates, £50 free of Duty.

" " Daniel Hanbury, £100 less Duty.

1876.

Legacy of the late Thomas Corbyn Janson, £200.

" " " Charles Lambert, £500.

George Bentham, Esq.: General Index to Transactions, vols. xxvi.–xxx.

1878.

Subscription portrait of John Claudius Loudon, by J. Linnell.

Subscription portrait of Rev. Miles Joseph Berkeley, by James Peel.

1879.

Rev. George Henslow and Sir J. D. Hooker: Contribution to illustrations, £35.

1880.

The Secretary of State for India in Council: cost of setting up Dr. Aitchison's paper, £36.

1881.

George Bentham, Esq., special donation, £25.

The same: towards Richard Kippist's pension, £50.

Portrait of Dr. St. George Jackson Mivart, by Miss Solomon; presented by Mrs. Mivart.

1882.

Executors of the late Frederick Currey: a large selection of books.

Subscription portrait of Charles Robert Darwin, by Hon. John Collier.

The Secretary of State for India in Council: Grant for publication of Dr. Aitchison's second paper on the Flora of the Kurrum Valley, £60.

1883.

Sir John Lubbock, Bt. (afterwards Lord Avebury).
Portrait of Carl von Linné, ascribed to M. Hallman.

Philip Henry Gosse, Esq.: towards cost of illustrating his paper, £25.

Royal Society: Grant in aid of Mr. P. H. Gosse's paper, £50.

Sophia Grover, Harriet Grover, Emily Grover, and Charles Ehret Grover: 11 letters from Carl von Linné to G. D. Ehret.

1885.

Executors of the late George Bentham, £567 11*s.* 2*d.*

Subscription portrait of George Busk, by his daughter Marian Busk.

1886.

A large selection of books from the library of the late Dr. Spencer Thomas Cobbold (a bequest for a medal was declined).

Sir George MacLeay, Bt.: MSS. of Alexander MacLeay and portrait of Rev. William Kirby.

1887.

William Davidson, Esq.: 1st and 2nd instalments of grant in aid of publication, £50.

Francis Blackwell Forbes, Esq., in aid of Chinese Flora, £25.

1888.

The Secretary of State for India in Council: Grant in aid of publication of results of the Afghan Boundary Delimitation Expedition, £150.

Dr. J. E. T. Aitchison, towards the same, £25.

Trustees of the Indian Museum: Mergui Archipelago report, for publication in Journal, £135.

Dr. John Anderson, for the same, £60.

Wm. Davidson, Esq.: 3rd and last instalment, £25.

Sir Joseph Hooker: (1) Series of medals formerly in possession of George Bentham; (2) Gold watch, key, and two seals belonging to Robert Brown.

1889.

Bronze copy of model for Statue of C. von Linné, by J. F. Kjellberg; presented by Frank Crisp, Esq.

1890.

The Secretary of State for India in Council: Grant for Delimitation Expedition report, £200.

Oak table for Meeting Room, presented by Frank Crisp, Esq.

Subscription portrait of Sir Joseph Dalton Hooker, K.C.S.I., by Hubert Herkomer, R.A.

Executors of the late John Ball, Esq.: a large selection of books.

An anonymous donor, £30.

Colonel Sir Henry Collett, K.C.B., towards the publication of his Shan States collections, £50.

1891.

Subscription portrait of Sir John Lubbock, Bt. [Lord Avebury], by Leslie Ward.

George Frederick Scott Elliot, Esq., towards cost of his Madagascar paper, £60.

1892.

Dr. Richard Charles Alexander Prior: for projection lantern, £50.

1893.

The Executors of Lord Arthur Russell: his collection of portraits of naturalists.

Electric light installation: cost borne by Frank Crisp, Esq.

1894.

Algernon Peckover, Esq.: Legacy, £100 free of Duty.

Miss Emma Swan: "Westwood Fund," £250.

1896.

Clock and supports in Meeting Room, presented by Frank Crisp, Esq.

1897.

William Carruthers, Esq.: Collection of engravings and photographs of portraits of Carl von Linné.

Royal Society: Grant towards publication of paper by the late John Ball, £60.

Subscription portrait of Professor George James Allman, by Marian Busk.

1898.

Sir John Lubbock, Bt.: Contribution towards his paper on Stipules, £43 14*s.* 9*d.*

Royal Society: Contribution towards F. J. Cole's paper, £50.

" " " " Murray & Blackman's paper, £80.

" " " " Elliot Smith's paper, £50.

" " " " Forsyth Major's paper, £50.

1899.

A. C. Harmsworth, Esq. [Lord Northcliffe]: Contribution towards cost of plates, £43.

Royal Society: Contribution towards Mr. R. T. Günther's paper on Lake Urmi, £50.

1901.

Hon. Charles Ellis, Hon. Walter Rothschild, Frank Crisp, Esq., F. D. Godman, Esq., and the Bentham Trustees: The Correspondence of William Swainson.

Royal Society: Contribution towards Mr. F. Chapman's paper on Funafuti Foraminifera, £50.

Prof. E. Ray Lankester: Contribution towards illustration, £30 5*s*.

Portrait of Dr. St. G. J. Mivart, presented by Mrs. Mivart.

1903.

Royal Society: Contribution toward Dr. Elliot Smith's paper, £50.

Legacy from the late Dr. R. C. A. Prior, £100 free of duty.

Mrs. Sladen: Posthumous Portrait of the late Walter Percy Sladen, by H. T. Wells, R.A.

B. Arthur Bensley, Esq.: Contribution to his paper, £44.

1904.

Royal Society: Grant in aid of third volume of the Chinese Flora, £120.

Supplementary Royal Charter: cost borne by Frank Crisp, Esq. (afterwards Sir Frank Crisp, Bt.).

1905.

Royal Society: First grant in aid of Dr. G. H. Fowler's 'Biscayan Plankton,' £50.

Executors of the late G. B. Buckton, Esq.: Contribution for colouring plates of his paper, £26.

1906.

Royal Society: Second grant towards 'Biscayan Plankton,' £50.

Subscription portrait of Prof. S. H. Vines, by Hon. John Collier.

Royal Swedish Academy of Science: Copies of portraits of C. von Linné, after Per Krafft the elder, and A. Roslin, both by Jean Haagen.

1907.

Royal University of Uppsala: Copy by Jean Haagen of portrait of C. v. Linné, by J. H. Scheffel (1739).

Royal Society: Third and final grant towards 'Biscayan Plankton,' £50.

The Trustees of the Percy Sladen Memorial Fund: First grant towards publication of Mr. Stanley Gardiner's Researches in the Indian Ocean in H.M.S. 'Sealark,' £200.

1908.

Prof. Gustaf Retzius: Plaster cast of bust of Carl von Linné, modelled by Walther Runeberg from the portrait by Scheffel (1739) at Linnés Hammarby; the bronze original designed

for the façade of the new building for the Royal Academy of Science, Stockholm.

Miss Sarah Marianne Silver, F.L.S.: Cabinet formerly belonging to Mr. S. W. Silver, F.L.S.

1909.

The Trustees of the Percy Sladen Memorial Fund: Second grant towards publication of Mr. Stanley Gardiner's Researches in the Indian Ocean in H.M.S. 'Sealark,' £200.

Prof. James William Helenus Trail, F.R.S., F.L.S.: Gift of £100 in Trust, to encourage Research on the Nature of Protoplasm.

1910.

Royal Society: Grant towards Dr. G. H. Fowler's paper on Biscayan Ostracoda, £50.

Sir Joseph Hooker: Gold watch-chain worn by Robert Brown, and seal with portrait of Carl von Linné by Tassie.

Prof. J. S. Gardiner: Payment in aid of illustrations, £35 0*s.* 6*d.*

Sir Frank Crisp: Donation in Trust for Microscopical Research, £200.

The Trustees of the Percy Sladen Memorial Fund: Third grant towards publication of Prof. Stanley Gardiner's Researches in the Indian Ocean, £200. (For third volume.)

1911.

The Trustees of the Percy Sladen Memorial Fund: Second Donation towards the publication of the third volume on the Indian Ocean Researches, £70.

The same: First Donation towards the fourth volume, £130.

1912.

The Indian Government: Contribution towards the illustration of Mr. E. P. Stebbing's paper on Himalayan *Chermes*, £46 15*s.* 2*d.*

The late Mr. Francis Tagart, £500 free of Legacy Duty.

The late Sir Joseph Dalton Hooker, O.M., G.C.S.I., £100 free of Legacy Duty.

The Trustees of the Percy Sladen Memorial Fund: Second Donation towards the publication of the fourth volume on the Indian Ocean Researches, £140.

The same: First Donation towards the fifth volume, £60.

1913.

Royal Society: Grant towards Dr. R. R. Gates's paper on Mutating Oenotheras, £60.

Sir Frank Crisp, Bt., Wallichian Cabinets, £50.

The Trustees of the Percy Sladen Memorial Fund: Second Donation towards the publication of the fifth volume, £200.

1914.

Royal Society: Grant towards Miss Gibbs's paper on the Flora of British North Borneo, £50.

ERRATA.

Page 59. Lines 15 and 16 from bottom, *read* "... was seventy-three years of age ..."

„ 61. Line 23 from top, *for* "... 6th March, 1914," *read* "16th March, 1914."

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Spolia Runiana II.—Results of the past Season's Dredging (Herdman), 2.
Stapf, Dr. O., elected Councillor, 22; elected Secretary, 22; on the late W. West's 'Ecological Notes, chiefly Cryptogamic,' 71; showed ball of raw rubber altered by seawater, 5; communications by (Oliver), 10, (Turrill), 14.
Stebbing, Rev. T. R. R., appointed Scrutineer, 22.
Strobell, Miss E. C., *see* Foot, Miss K.
Sudân, Southern, Non-Marine Mollusca from (Longstaff, etc.), 4.
Sutton, A. W., on Dr. Lotsy's 'Origin of Species,' 96.
Sutton, M. J., deceased, 17; obituary, 63.
Swynnerton, C. F. M., Short Cuts by Birds to Nectaries, 11; Short Cuts to Nectaries by Blue Tits, 70.

Tagart Fund, purchase of books from income from, 1, 8.
Tasmania, *see* Chilton, Prof. C.
Telephorus lituratus, Life-History and Structure (Payne), 71.

Tentacles of *Blennius gattorugine*, 8.
Terrestrial Isopoda, *see* Isopoda.
Thomas, Miss E. N., elected Councillor, 22.
Thomas, H. H., and Miss N. Bancroft, Cuticles of some recent and fossil Cycadean Fronds, 2.
Thomson, Miss A. W., admitted, 11; elected, 3; proposed, 1.
Tieghem, Prof. P. E. L. van, deceased, 18.
Tillyard, R. J., Life-Histories and Descriptions of Australian Æschninæ, 71.
Towndrow, R. F., proposed as Associate, 2; not elected, 5.
Treasurer elected (H. W. Monckton), 22.
Tremearne, Mrs. M. N., admitted, 17; elected, 15; proposed, 12.
Tristichaceæ. A New Natural Order (Willis), 16.
Triticum dicoccoides, Koern., from Palestine, exhibited (Percival), 3.
Turrill, W. B., Contrib. to the Flora of Fiji, 14.
Tweed, River, *see* Hayward, Miss I. M.

Utakwa Expedition, Botany of (Ridley), 19; lantern-slides illustrating (Wollaston), 16.

Vancouver Island, an Account of a Journey into its Interior (Mrs. Henshaw), 8.
Veitch, J., & Sons, exhibition of cross-grafted plants, 4.
Venkata Rau, M., elected, 16; proposed, 12.
Vice-Presidents appointed, 68.
Vienna Rules, Art. 45 (Druce), 8.

Waby, J. F., withdrawn, 18.
Walker, A. W., deceased, 17.
Walker, Comr. J. J., elected Councillor, 22.
Wall, Major F., elected, 14; proposed, 11.
Wallace, A. Russel, deceased, 17; death referred to, and vote of condolence passed, 2; obituary, 63–65.
Weiss, Prof. F. E., on Dr. Lotsy's 'Origin of Species,' 95.
Wernham, H. F., elected, 11; proposed, 7.
West, W., deceased, 17; obituary, 65–67; Ecological Notes, chiefly Cryptogamic, 71.
Wettstein, Prof. R., elected Foreign Member, 16; proposed, 11.
Wheat, wild, specimens from Palestine exhibited (Percival), 3.
White Island, New Zealand, its Vegetation (Oliver), 10.
Williams, Rev. W., deceased, 17.
Willis, Dr. J. C., A new Natural Order of Flowering Plants: Tristichaceæ, 16.
Wing, W. E., some of his drawings of British insects exhibited, 10.
Withdrawals, 18.
Wollaston, A. F. R., showed lantern-slides illustrating Utakwa Expedition, 16.
Woodward, Dr. A. S., elected Councillor, 22.
Worsdell, W. C., showed lantern-slides of Cape plants, 10, 14.
Wyss, Miss C. von, elected, 70; proposed, 15.

Yapp, Prof. R. H., withdrawn, 18.
Young, Dr. A. P., appointed Scrutineer, 22.

Zoological Secretary (Prof. G. C. Bourne) elected, 22.

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APPENDIX.

A Reprint of the Booklet by G. W. Sleeper, dated 1849, and forming the Subject of the Presidential Addresses in 1913 and 1914.

The work has been printed as in the original, page for page and line for line, and the type has been matched as nearly as possible.

All the mistakes of the original have been preserved, with the exception of a few letters or hyphens which have slipped at beginning and end of lines. These instances are noted below :—

Page 1, l. 5.	all- (*raised hyphen*).
,, 3, l. 15.	infinitesimally (*raised* i *at beginning*).
,, 3, l. 24.	characteristics (c *lower down*).
,, 4, l. 4.	conception, (*comma lower down*).
,, 5, l. 11.	il (*slight space between* i l).
,, 5, l. 27.	species, (*raised comma*).
,, 8, l. 11.	in (i *lower down*).
,, 8, l. 15.	germ, (*raised comma*).
,, 11, l. 6.	dis- (*raised hyphen*).
,, 12, l. 15.	un- (*raised hyphen*).
,, 15, ll. 3, 4.	ing . . . ly (i *and* l *lower down*).
,, 15, l. 9.	ignorant (i *lower down*).
,, 16, l. 18.	ish (*raised* i).
,, 19, ll. 1, 2.	not . . . the (t *and* e *lower down*).
,, 23, l. 1.	re- (*raised hyphen*).
,, 24, l. 27.	hu- (*raised hyphen*).
,, 25, l. 26.	ier (i *lower down and slight space between* i *and* er).
,, 27, l. 12.	fi- (*raised hyphen*).
,, 29, l. 13.	having (*slihgt space between* n g).
,, 31, l. 4.	men- (*raised hyphen*).
,, 36, l. 5.	cre- (*raised hyphen*).

SHALL WE HAVE COMMON SENSE

SOME

RECENT LECTURES

WRITTEN AND DELIVERED BY

GEO. W. SLEEPER.

BOSTON:
WM. BENSE, PRINTER.
1849.

Entered according to Act of Congress, in the year 1849, By Geo. W. Sleeper, in the Clerk's office of the District Court of Massachusetts.

INTRODUCTION.

The original ideas on which these lectures were founded, entered my mind when I was barely seventeen, and during their gradual development the major portion of both of the lectures were committed to paper in the course of the past six years. The first public delivery of either occurred in Boston about two years ago, it was, however, almost impossible to obtain a hearing for them so bitterly hostile did the audiences (assemblies little better than savages) prove themselves to be, and so persistently did they interrupt, and endeavor to overawe, the lecturer.

Such being the condition of society, and as the press of this city, influenced by certain clergymen, have not seen fit to acquaint the public either with the substance of the ideas set forth or with the facts relating to the attempts to deliver the lectures containing them, I decided to put these subjects to print; as being the only way open to secure attention from the many fair and noble minds among our citizens who undoubtedly have Common Sense.

The Origin of Life

AND THE ADVANCE OF CIVILIZATION.

Let us take a cursory survey of an abstruse subject, one that has naturally woven an ever-fascinating, a mind-stimulating and an all-powerful spell over the vast majority of the deep thinkers of past and present ages.

Born out of the womb of Night; dwelling in the house of Night; and fading away in the embrace of Night—how mysterious, how difficult, how remote is this interesting phenomenon: The Origin of Life, on which scientific and speculative Man vainly ruminates and theorizes while borne dreamily along on a hastening current of hopeless perplexity and confusing course; and yet on which a so-called God-inspired Bible assumes to expound with authority and to relate with truth!

In this legend Man and all the Animals are introduced as creations of a single day! Geology has dispelled that illusion from minds

open to admit the irradiating light of Knowledge. And it is also fabled that all this work of Creation occurred but six thousand years ago! Which Creation, the state of civilization in India and China some five thousand years back, is sufficient to disprove without the abundant aid derived from Geological evidence; all proving an inconceivably ancient history for Man.

Leaving the crude, brief, unsatisfactory and incredible account of the Creation given in Genesis to its inevitable and unregrettable fate, as one of the most audacious insults ever offered to the human intellect; a reasoner is irresistibly drawn on to conclude that Man has been impelled, as Education gradually expanded his growing intellect and drew him away from the evil grasp of the deceiving priest in the dense shadow of the enslaving church, to read with more and more deductive attention the open book of ingenuous Nature under the blue dome of a clear sky in the frank light of day.

Thus has he been broadeningly led to inspect the vastly altering geological strata, to compare the multi vegetal transformations occurring from age to age, to meditate upon the

remarkable fossils re-constructed by the amazing genius of Cuvier, to study the involved appearances, disappearances and relations of long bygone or of still living races of barbarous and civilized mankind and also of species, varieties and individuals of animals, plants and minerals in their likewise complex interrelations, differences and ramnifications: aye, until even like Baer he discovers the true seat of the human germ of Life.

As a grandly final result of his readings, his reflections, his searches and his experiments he finds that Life owes its faint beginning to primal germs. These germs I hold to be infinitesimally minute living atoms pervading the entire terrestrial atmosphere; and, perhaps, the entity of the Cosmos.

Perishable themselves, each is the common carrier of the principle of Life which is indestructible and eternal, and each is lastingly imbued with the peculiar characteristics of the species and, but less so, of the individual its particnlar variety alone is able to form. The characteristics and attributes of any germ may be greatly modified or limitedly altered during its union with its sexual opposite to form its kind,

Arranging themselves in strict obedience to dominant Natural laws of which we have but little knowledge, in a vastness of Space of which we have no intelligent conception, and exhibiting an almost unmeasurable minuteness reduciug our feeble powers of comprehension to appalled nothingness, these germs are slowly developed, altered and perfected during an immensity of periods of time compared to which the six thousand years of the Bible is but a trifling era—a pebble on the sea shore; a tiny star in the Milky Way.

These germs of life and the individuals growing out of them, were constantly experiencing change. Weakened by exposure and attack they were only preserved unmodified for a brief (though variable) period by means of the more or less effectual resistance they were able to offer to climatic conditions, changes of food, convulsions and transformations of Nature and the destructive inroads of overpoweringly hostile forms of reptile, piscine or animal Life For example: just as most of you have beheld a neglected flower perishing in the midst of over-topping weeds or languish ing for lack of the vital sustenance that grass or clover has withdrawn from its starving roots

How these tiny germs first creatively grouped together, at Nature's will, out of the vast chaotic void; what the first formed creature was and what amazingly prevailing law compelled their more passive obedience to its mute behest than ever the humblest of the nations abjectly rendered unto the loudest mouthed decree of imperial Alexander; will probably remain forever enshrouded in clewless mystery darkly closed as the burned tomes of the Sybil to the anxious inquiries of investigating Man. I should venture to imagine, taking the researches of Leeuwenhoek, of Swammerdam and of Schwann in consideration together with the results I have tediously arrived at by microscopical and generative experiments, that germ cells of the simplest form at first existed; that these obeying the Unknown Cause I have alluded to, gradually extended themselves branching out like the Banyan tree, but to generate, or metamorphose into, more or less progressive species or varieties; the lowest the next higher, and so on, trilobite to mammal: one of the many branches necessarily terminating temporarily in a triad: the Lemurs, the Monkeys, and—Man himself.

Thus appeared the original of our species,

not fashioned out of clay or after God's image; originally naked, savage ravenous, lustful, unsympathetic and revengeful, he has never retrograded, but at all times in one portion of the world or another has steadily advanced, raising himself ethically and intellectually throughout the course of the ages entirely without the aid of supernatural revelation or divine assistance, simply by his own increasing Reason and that Experience which so admirably cultures the tender flowers of Morality while eradicating the hardy weeds of Vice; and also by his own persistent efforts stimulated by Education,—but always, though more or less hairy and uncouth, he was, nevertheless, essentially Man.

And as such he cannot but remain.

I am well aware, Ladies and Gentlemen, when I make the broad statement that Man and the Ape are co-descended from some primary type, that there is little resemblance in face or figure between the nude forms of the symmetrical, pleasing, beautiful Apollo and Venus that the surpassing art of Apelles and Praxiteles has richly bequeathed to us and the ungainly, loathly and ugly contour of the Simian tribes; but remember that it is not to the higher evolutions of Man nor to the low-

er variations of the Monkey that we must look in this matter, but to just the reverse of each. And then in many respects a striking parallel is observed to exist between the "lord of creation" and his Quadrumana relatives the Chimpanzee and the Ourang Otang which clearly establishes the extremely rational view I have advanced and enables us to experience very little difficulty in tracing back these kindred creatures to a remote, an ancient, but a general stock; although exactly what that common forefather was is as likely to forever remain as profound a mystery as the astounding problem of how the beginning of things could take place or the absolute ending of all matter can ever occur!

We have never found any living or fossil link to form a chain between Man and the Ape, nothing to close the gap separating the two related animals like Owen's rhynchosaurus bonds the avines to the saurians, and the pliosaurus unites the pterodactyle to the crocodile. I doubt that any such creature will be discovered; to my mind there never was occasion for its existence: more likely some relic of an, at present, unknown animal proving to be the forerunner of both species, will be ex-

humed out of the debris of an age immediately antecedent to both: just as the gigantic mammoth was dug out of the congealing bosom of Siberia's ice in 1800 and the three-toed palæotherium, rude ancestor of our valued horse, was disinterred from the profound of the Eocene in earlier years.

The wonderful progressive presentation of forms ascending from day to day in the scale of organized beings exhibited by our species in the fœtal state and remarked by the physiologist, is another substantiation of my theory

Man, in the womb, passes through all the embryo forms of the types of the known divisions of animated Nature: first but a germ, he then resembles mollusk, fish, reptile, bird, rodent, ruminant, and batrich, finally assuming unmistakeable features of the human animal. Thus we see a significant panorama of the momentous changes which occurred in the course of ages displayed in the short space of nine months in the mysterious womb.

Primal matter to zoophyte, to crustacean, to fish, to saurian, to mammal and to Man! the inferior forms disappearing when their allotted work was done and the improved era no longer warranted theirthen inutile existence

just as the platysoma of the Old Red Sandstone vanished in the Magnesian Limestone; or else becoming dwarfingly modified previous to extinction: similarly to the diminution of the seventy foot iguanodon into the four foot and perishing iguana which has outlived the age and environment which called its giant ancestor into being.

In this ever recurring disappearance, or modification, of animal and vegetable forms there is nothing strange. Everywhere about us we see waged the pitiless battle for life of which it is the inevitable outcome. Just as the dandelion, the plantain and the burdock extend their broad leaves o'er the verdure of the meadow, starving and exhausting the grasses around them; just as the weeds of the garden o'erwhelm with rank luxuriance the feebler cultivated vegetables during a brief period of neglect; and just as species of birds and animals become extinct from their merciless slaughter by barbarous man who spares not his own weaker kind, varieties of his race thus fading from sight; so there is a continual process of elimination and substitution going on in the great laboratories of Nature; the use-

less perish, the useful live and improve, although our conception of what is useful and improving is often opposed to Nature's.

All Geology indicates assured progress by its presentation of higher and higher generations of exalting forms, each remarkably suited to the peculiar age in which it appeared at its best. Of all such forms, that of Man would seem to be the masterpiece of Nature on this globe, and we cannot fail to be impressed by a conviction that whatever Nature may have accomplished on other planets, all her energies upon this have evidently been exerted, through the medium of Education, to shape, to instruct and to perfect, the Human Race.

But our parallel of comparison of Man with the Simians is effectually arrested by a three fold, irremovable, barrier: the possession of progressive, deductive and inventive Reason; the acquirement of extensible and transmissible Speech; and the innate knowledge of the formation and use of Tools; attributes resident solely in the spermatozoa and ovum of the human species, and absolutely inseparable therefrom. So I am spared the unpleasantness of announcing anything so distasteful, to you, as

that the monkey was parent or progeny of Man; thus, this Scylla and Charybdis being happily steered past, I merely find it necessary to reaffirm my fixed belief in the gradual origination of both from a general parent, each distinct from birth and each preserving that distinctiveness unto the present epoch, modified somewhat, it is true, in the instance of the human individual owing to his germ-endowment with the rudiments of primarily cultivatible and incalculably higher functions.

I feel assured that skeletons or rock-preserved hairy specimens of this ancestor of the two species will ultimately be excavated from the strata preceding that in which remains of Man and of the Monkey have been found, to at once and forever confound the silly, the incredible, the sense-offending relation of the Book of Genesis; hashed together and served up to the simple by posturing and imposturing knaves of priests. The greater perishability and much more restricted production and distribution of the human frame in those remote ages, together with the very limited extent of the exploration of this but recently thrown open field have so far precluded success.

Thus, then, was Satyr Man. Behold him!

Education took this beastial, crude and ignorant Frankenstein horror by the clumsy hand and kindly began to lead him on in ever-gaining pursuit of the lofty ideal which alas we yet see flitting before us in the brightening distance, illusive, fanciful, unattainable, will 'o the wisp like, still. But nevertheless no chimera, like poor Ponce de Leon's Fountain of Youth, but a glorious reality, a luminous to-be of the glad time to come.

And her kindly care, Mentoring a Telemachus, which began with Man's stormy advent on this troubling planet; has never ceased, but has multiplied and multiplied, solicitously training the uncouth fingers of the undocile creature first to difficultly shape the roughly-sharpened knife of wood; next to fash ion the weapon out of fractured bone or shell; then to discard these for laboriously ground flint or stone; giving place, in turn, to more durable bronze or copper and yet again to the keener and harder iron or the tempered steel; improving the quality, increasing the number and extending the usefulness of her pupil's weapons and tools. Enabling the civilizing savage to exchange the damp, smoke filled, gloomy, bone strewn cavern for the dry, ven-

tilated, cleanly mansion of wood, brick or stone elevating him to lofty distaste for the red fields of fiendish fray and sickening slaughter, the grim cannibal feast of flesh and blood and the disgusting orgies of sexual obscenity and drunken madness that characterized his earlier ages—insensibly drawing him slowly on to display kindness, sympathy and toleration towards his Brother Man.

Yes! Education, benificent Power, thou hast produced all of the "Material" whereof we of the earth have cause to be thankful for. And thou wilt produce incrdeibly more than even the most sanguine of us can venture to predict; besides incalculably ministering towards the true "Spiritual" of Man—not the ghostly, the priestly, the false intangibility of which knaves or fools so glibly presume to prate. For I disbelieve that in the marvellously complex piece of mechanism which we call Man (moved by some unknown Force pervading the Universe of which force electricity and chemical action are but a few of the observable effects) resides anything more "Spiritual" than can emanate from this Force: a Force which is manifested in the working of our brains and in the involuntary movements of our bodies.

Spiritualism plays no part in the great drama of this our life. Indeed, the sole phenomenon approaching to such is the departure of the life-germ from a perishing frame but to begin anew in another or an allied form. For nothing is lost in the vast economy of Nature: all things suffer but a transient change of form: thus the life germ resident in Man transmitted to his descendants, goes on existing indefinitely throughout all Time's infinitude of years; just as his Thoughts, those Genii children of his prolific brain, continue to live on ages and ages after that transitory organ has mouldered away into the dust of the long ago; and thus, and only thus, is Man immortal: in the transmission of his germ life and in the eternal perpetuation of his Original Thought.

Ladies and Gentlemen, pray excuse this long digression; I now return to my subject: I was remarking how Education had abated the savage and revolting appetites of Man, how she has fanned the flickering flames of melting mercy and soothing sympathy and sincere Brotherly love—qualities almost wholly wanting in the eldest of Earth's peoples—how she has dimmed the furious fire in the persecuting eyes of the religious bigot; quenched the

fierce flare of his flesh consuming faggots and rusted for aye the levers of his muscle-rending racks; how it has raised the poor, the lowly and the ignorant and brought the rich, the high born and the learned down to a more common and less caste marked plane.

It has also insensibly, yet surely, softened the rigor of a host of heart breaking evils that ignorant men bore patiently, apathetically or helplessly in the long, dark, dreary times agone, simply because they knew no better, or adventured to breathe no murmur, or dared not rub the talismanic ring of Change—until Education with her Muses of Poetry and Prose; her Commerce and Travel; her Printing Press and Schools; her Gunpowder and her Agnostic with his untrammelled Thought, pointed out to them the very place to set the chisel of Emancipation upon the formidable gyves of venerable Tyrant Custom and ancient High Priest Superstition, and told them when to drive the keen edge through a rusty link with the Thor hammer of Progress wielded by the lusty arms of the Thinker of Free Thought. Alas! that even in this, the freest and fairest of all the countries on the Globe, the needed work should as yet have been so

partially and haltingly performed: The taint of Slavery still adheres to us with rank repulsiveness and sinful shame. Abolished by England, France and Holland it is left for the land of freemen to stand shoulder to shoulder with Turkey and Muscovy in the rear ranks of Civilization's columns; its heel on the breast of the hapless Negro precisely as Muscovy grinds hers on the bosom of the cringing Serf.

Education, that must set both free, has yet to accord to us that Freedom of Speech, that Equality before the Law, now wrongfully denied to the poorer and weaker and the irreligious of the citizens of the World.

And to abate the lengthy hours of toil, the saddenning overwork of unjustly kept down women and stunted little children. And banish the brutishness of the majority of the people, the arrogance and frivolity of that minority: the rich and great, and the frenzying influence of the Arch Fiend, Rum. Above all it must exorcise the horrible nightmare Religion from every priest cursed land; substituting the pure, the noble, the holy creed of Thomas Paine: the Doing of Good to all in that common World which is our universal country. Thus shall we answer the problem

of the wherefore of our existence as far as we can elucidate it; and thus reconcile ourselves to its certain termination, Death. Of what lies beyond the grave we know nothing—Death is a mystery we may never hope to solve; But he, whatever his race or creed, who tries to do right to all, need not fear to die.

Education has yet te remedy a multitude of evils besides those I have touched upon—civil, social, sexual and political; which cannot be entered into in the compass of this lecture. And she will do all this and more in the years to come; years which for her are eternal.

A great assistant to Mother Education in the tedious licking into shape of her bear-cub Man will prove to be the dawning but pregnant Science which the peculiar physiognomy of a schoolboy awakened into perception in the brilliant mind of the sage Spurzheim, later to be developed by the reflective Gall.

I allude to Phrenology, which young as I am, I have studied for the past six or seven years. I know it is a science at present held up to ridicule by the superficial, the unthinking and the ignorant; it is one, however, certain to become one of the most potent instructors of the race—one of the most tried and

trusty staves Man has to lean upon for guidance and support. I cannot here enlarge on this miscalled "science of bumps" so despicably attacked by the press in England and in this country—bumps, in fact, it has very little to do with, being really a science dealing with the measurements of angles and the thinness or thickness of parts of the skull. But at a future time I intend to devote an entire lecture to a subject so prodigal of light upon Man's self-means of education and perfectitude and so certain to give rise to greater progress and greater results. Briefly: in my opinion, the study and application of Phrenology is essential to the civilization of Man.

I must soon conclude; but before so doing I have to remark that I am quite aware that a distinguished Frenchman, Lamarck by name, many years ago suggested that there was a gradual and progressive alteration of animal types, but he founded his ingenious theory entirely on Spontaneous Generation, which I intend to demonstrate in a succeeding lecture has no substantiation, and he (not enjoying the geological, chemical and phrenological advantages that increasing Knowledge has showered on us of the present century) but very

imperfectly outlined the subject and did not conceive my principal ideas. I may say the same of the anonymous author of a book called the "Vestiges of the Natural History of Creation" which appeared about three years ago. The book is in some respects a remarkable production and would possess greater merit if its author was not so lacking in scientific knowledge as to place blind faith in a Personal Diety and its benificent planning of the Universe, and to credulously accept the most unlikely tales of animal and vegetable marvels, unconfirmed and poorly vouched for, that one has read outside of the works of Munchhousen and the Brothers Grimm. Both found a lock but neither found its key. Whether I have discovered that key or not, must rest with you and your acquaintances or posterity, Ladies and Gentlemen, to decide—your most careful consideration of what I have set forth being earnestly entreated; for:

"Truth, like a single point, escapes the sight
And claims attention to perceive it right;
But what resembles Truth is soon descried,
Spread like a surface and expanded wide."

The Dangers of the Unseen.

During the few, brief years of our existence in this wonderful world, we sre repeatedly indebted to the constant, the resistless and the enlightening advance of Education for some all too fleeting, yet mind-enlarging, glimpses into the profound, the exhaustless and the enchanting book of Knowledge; that book which, although freely opened unto everyone by the liberal hand of Nature, would have been consumed, annihilated, or at least forever sealed to the curious gaze of priest-ridden Humankind had a bigoted, enslaving and persecuting church possessed the power.

We must not wholly blame the clergy for this would-be wrong. For, in general, it is unthinking Man who wilfully or stupidly places himself in the ready way of his own advancement; who drowns the feeble voices of the gifted few who have something new, in-

structive, or uplifting to impart for the lasting benefit of him and of his race, with the hiss, the groan, the loud uproar, of the ignorant barbarian; and who in his lamentable shortsightedness casts back the beautiful, naked, mind culturing Truth into the depths of her obscuring well—just as the stern, fanatic, recluse saint thrust the lovely maiden who sought his unresponding love, from the aweful precipices of his mountain home lest her too seducing charms should wean him from the austerities, the veneration and the dominion of God and the Church.

Well it is for us that Truth is more than mortal; unlike the poor, feeble girl of the sorrowful legend. Dazzling, pure and fearless, she emerges again, again, and ever, from the depths of crystal clearness as might a twin-goddess—a Minerva-Venus—from her natal wave, each time fairer, stronger and more prevailing; refreshed e'en as the o'erthrown Anteus when, re-invigorated, he upsprang from the bosom of his Mother Earth. Aye, and it is fit that this should be so, for only such incessant, such dauntless, such thought compelling persistence can eventually command the admiration, captivate the mind and impreg-

nate the brain of Thought retarding, God fearing, Church crippled Man; brush the dust from his blinded eyes and disclose to him the majestic thoroughfare stretching grandly out to the shining Temple of Progress, an Olympus the poor dupe who tamely submits to the dark guidance of Religion can never see.

Original Thought, fair thriving child of prolific young Dame Education, has found a deadlier enemy than ever adventurous, valorous, wrong redressing Amadis o'ercame in foul, fire breathing Dragon of the Myths of yore. That foe is Holy Church, whose Titan power, whose Briærius reach and whose Machaivellian craft at one fell period of theworld's sad history almost blotted Intellectuality out of human minds, laid struggling Science on a Cæcus' bed, and infamously halted the sublime march of divine Progress on its difficult but inevitable way.

It is this Original Thought, cradled in some Crichton's brain, that has exquisitely chiselled the civilized (or rather the civilizing) Man out of the rough barbarian; that is irresistibly, openly and enlighteningly overthrowing the Colossus of the Church; and that more and more conclusively demonstrates in the minu-

tiæ of the smallest things it presents to our receptive minds, the potency, the majesty, the unchangeability of the Laws of Nature as luminously contrasted with the impotency, the puerility and the contradictoriness of what is audaciously related of a Biblical personal God.

Original Thought, unaided by sage Experiment, remains but lame Theory—a child in close leading strings—and can be accepted as mature Fact or not, as he to whom it is conveyed, suggested, or expounded chooses to finally decide. But let it be backed by the strong confirmation of Experiment: it then becomes Practical—it is Fact itself, and sooner or later must be received as such by even the most doubting, scoffing and bigotedly oppositional of our obtuse kind.

The Original Thought I wish to put before you tonight may be, I am sorry to say, devoid of as much of the potent aid of Experiment: that is, systematic, directly planned and skillfully conducted Experiment as might be wished. Yet it is a clear expression of Natural, observed phenomena, supported by simple but certain proofs; therefore it presents definite claims to be thoughtfully received as no vagary of Theory, but a substantiality of Fact.

From observation, thought and deduction, I have concluded that earth, air and water are replete with multitudinous, infinitesimal, ever active creatures, probably many of which are too minute for our best microscopes to reveal to the sense of sight. These pigmy, insignificant-seeming and yet collectively formidable beings are everywhere at work—born in myriad numbers, propagating and expiring in a brief season, like May-Flies, ere they hardly have begun to live; they infect or disinfect the air, alter the nature af the soil, and cleanse or contaminate the water. Entering our systems through all of these facile mediums (we inhale on an average about 2800 cubic feet of air through our lungs in the short space of only twenty four hours and swallow dust and water in surprising quantities every day of our lives) they soon obtain mastery over our weakened bodies and it is surely their malefiant effects that we behold in the horrid virulence of small-pox, the dreadful malignancy of cancer and the melancholy ravages of consumption. Kings evil, catarrh, malarial and scarlet fever, measles, the putrid sore throat, gangrene, aye, dozens of other catching ails let loose from a cursed Pandora's box among hu-

mankind, are simply the direct or indirect re sults of these almost invisible atoms.

The drop of water we sip, the breath of air we breathe, the particle of dust that finds insidious entrance to our bodies, may be to us the unseen precnrsor of terrible disease, the unheard knell of fateful doom, the silent fore runner of early, torturing, ghastly death.

Only nearly perfect, very healthy, vigorous bodies can successfully resist these parasitic wanderers, (Ishmael-like roamers through the universe) when they are at their best and we are exposed to their attacks: hence we note a favored few who defy plague and pox and cholera; epidemics of all fateful kinds and remain serene, incautious and unscathed. But wo to the weak, the ailing or the debauched! they fall an easy prey to the fatal onslaughts of these tiny destroyers, everywhere watchful, voracious, insatiate; even as the Harpies of fable or the vulture that preyed on Promotheus. Why this is so, I am unable to say; it is doubtless for the same reason that a stunted, set back, or sickly piant yields to the massed attacks of aphide hosts while healthier close by vegetation of the same kind is nearly free from the vermin destroying the

weaker foliage: from what either results, our too scanty store of knowledge avails us not to tell. It may be that, precisely as the spermatozoa, which Leeuwenhoek in 1677 discovered existing in the bodies of men; so other living germs, antagonistic to the evil kinds, may oppose the latter in undebilitated systems and wage war upon the humours in our blood.

I am aware that this mere idea of things such as living atoms produced by the forces of Nature is, in itself, by no means novel.

The wise Aristotle conceived that the mud of rivers might become impregnated with active life by the wizardry of Spontaneous Generation and thus give birth to fishes. Then you will recall to mind the old fable of the bees whom Sampson found abiding in the lion's carcass. Oh how bitterly the church attacked the presumptuous few who first timorously ventured to question the validity of the grounds for this utter absurdity! to deny the statement that neither germs or eggs were nec essary for the production of the insects! But the churchmen got a serious set back early in the seventeenth century when Redi laid bare the surpassing fallacy of their claims by making known his justly celebrated experiment

with pieces of flesh kept in covered and uncovered jars; and again it tottered when Spallanzani followed in Redi's track with incontestible proof of the continued barrenness of a well boiled infusion preserved from contact with air by a sealed glass vessel. Neither investigator was able to detect the faintest trace of life after long and patient observation. Yet such a man of genius as Lamarck believed in Spontaneous Generation and many others fell in with his views, indeed it required Schwann's truly scientific and masterly researches to finally disprove the plausible theory in 1837, scarcely eleven years ago.

I have repeated most of these interesting experiments and in addition to these have made many more of my own devising. to attempt to now detail which would tediously and uninstructively prolong this, I have no doubt to you, seemingly already drawn-out Lecture.

From them I find that Schwann is, in the main, correct; but further note that the close sealing he has recourse to is unnecessary, mere ly stuffing the mouth of the containing vessel with cotton wool will answer the same purpose: that of a germ barrier, as the most elaborate sealing if the heated wool is held in a forceps.

A hood dipping into quicksilver answers excellently. Yet mere filtration is inadequate.

I am now certain that this much talked of spontaneous generation is largely a delusion and that if the phenomenon ever does take place it is only exhibited under extremely unusual conditions and in most extraordinary cases. I am also aware that Kircher ascribed the virulence of the Plague to the action of living creatures—you will find his assumption elaborated upon in his scarce and curious "Scrutinum Contagiosæ," published nearly a century and a half ago—but he entirely failed to prove his theory by recourse to experiments (I have made many) and deduce from them many things they made manifest to me.

It is one thing to hint, to guess, to prophesy, but quite another, I assure you, to be able to truthfully say "I know," and then to connect the vague and formless spirit of the Theory with the definite and tangible Matter of Fact.

This I may modestly assert I have done.

I have placed some of the matter containing certain of what I have found to be the minute beings causing the putrid sore throat in the common calves foot jelly so familiar to all of you and covered this with a bell jar

standing in quicksilver; jelly, jar and metal all being strongly heated—thus, as you can readily understand, completely excluding any other germs. In three days, these particular germs had amazingly multiplied as rounded jointed grains in wavy clusters, and each little portion of the jelly when placed in pieces of fresh jelly was capable of plentifully propagating them in the new material which, in turn, produced them in others—and so on.

I was curious to see how they would adapt themselves to human blood: to avoid the red color I let some venous stand quietly, having skimmed off some crust; and drew away the greenish yellow watery serum from the cruor and heated it for some hours to 140 degrees of Fahrenheit which I have found sufficient to destroy germ life. In this the beings appeared in only thirty hours! and were larger, fibrous and moist. Other children in the same house with the boy from whose throat I obtained the matter, yielded none of the peculiar germs referred to—until some acquired the same disease, these then exhibited the noxious organisms in his or her greyish throat matter.

Reflection on this, leads us to consider how many diseases may be introduced, transmitted

or conveyed by the contact of flesh, perspiration or saliva and how cautious, how prudent and how circumspect we should ever be in governing the intercourse of ourselves and of our children with other persons or creatures, no matter whether the latter appear to be clean and healthy or not. If the system is in proper condition one will go as safely through any contagion as a diver in Halley's suit through the water of the sea, but who is so wise as to know when he is absolutely hale and hearty? one fatal moment of inviting weakness, and wo! the insidious germ has found a foothold.

Then comes the difficulty of dislodgment. These creatures floating numerously through the entire atmosphere and only able to thrive in a congenial harbourage: just as the Tobacco Worm can subsist but on the foliage of the plant from which it receives its name—once having effected this subtle lodgment in our systems, are there protected by phlegmatic or viscid excretions partly emanating, perhaps, from their own organisms and partly consisting of corruption thrown out by the irritated tissues for the purpose of resisting the invasion: as observed in putrid sore throat and in catarrh; and then withstand many substances

that otherwise would immediately kill them.

Do not misunderstand me: I do not mean to say that all diseases are thus contracted, but only those that are similar to those I have mentioned. Nor do I assert that the observance of all the precautions hinted at, will ward off all danger. Careful observation has led me to perceive that many of our common insects are conveyers of the germs of illness, disease and death. The House-Fly that crawls over our food or drowns itself in our drink, the Gnat that buries its lancet in our veins, the Bed-Bug that attacks the uncleanly during the slumbers of night, the Wasp and the Bee that thrust us with their stings—all are transporters, injectors or introducers of germ life.

My reasons for affirming this are briefly comprised in the statement that I have never known a person to suffer from malarial fever that had not at one time or other been stung by Gnats or been the resident of a region containing these pests; and that anyone can appreciate the too palpable filthiness of the Fly, feeding, crawling, rioting and breeding in the loathsome repulsiveness of refuse, offal, and decaying abominations of every conceivable kind; then alighting on our victuals or on our

persons to trail his defilement over our absorptive epidermis. Doubtless many of you, Ladies and Gentlemen, have seen a magnification of the foot of the Fly, you will therefrom comprehend how easily the wonderfully formed suckers by which this curious creature is enabled to support its own weight in an inverted position, can take up and transfer and impart germs of every description; and you will recognize what extraordinary facilities the insect possesses for acquiring and delivering them.

I suggest that wooden frames be covered with metal gauze such as is used for sieves, and hinged on outside of doors and windows, otherwise, with free access to your houses, what can be safe from the Fly? Oldys, Ladies and Gentlemen, penned a quaint poem on the Fly, bidding the latter "Drink with me and as I;" but I am far from sharing his peculiar poetical sentiments, and I believe that could the "Busy, curious, thirsty" insect be swept altogether out of existence the world would be ever so much the better for it. How to accomplish this, and also annihilate the swarms of Ticks, Fleas, Gnats, Bed-Bugs, and so forth, is indeed a question. Of course cleanliness will effect something towards the desired end; but, per-

haps, more than this may be called into play: maybe other creatures can be bred that may abate the numbers of these vermin, just as the Mungoos is used by the Hindu to destroy the Cobra-di-Capello, as the Ferret is set upon the Rat in England and the Ladybird is said to be employed in the South of Europe to exterminate the larvæ of various insects that prey on the poor peasant's scanty crops. Indeed, this is a fascinating, an exhaustless, a labyrinthine subject and could be enthusiastically pursued like Ariadne's silken clue, with infinite benefit to our suffering race. We have an approach to something of the kind in Jenner's valuable vaccine for the prevention of Small-pox. Here, I believe, is shewn a striking instance of the destruction of one germ by another: the substance he employs must contain creatures antagonistical to those of the Small pox pustules, else how could its effects endure so long in the systems of those inoculated with it? And that this, supplied in sufficient quantities, not only overwhelms those of the malady when they enter the blood, but continues to exist, inhabit and breed in that fluid thereby preventing entrance of the other and fatal germ life. If this is so, then what a Sun

of Hope—Hope that should "Spring eternal in the human breast" bursts in dazzling splendour on afflicted Man: consumption, cancer, cholera, all must mitigate, recede, or vanish before benificent germs meeting the horrible, destructive causes on their own grim ground and remaining triumphant on the never again to be fought-for field. You may laugh at this, and call it the rosy imagery of a too exuberberant fancy; but let me ask you to observe and notice that things much more incredible have come to pass, aye, and are occurring daily about us, that are now—like the steam engine the great Strassburg clock and the marvellous discoveries in chemistry—so accepted and commonplace as to remain almost unnoticed: what we deem the impossibilities of one age are the actuallities of the next, and I am thus explicit in bringing what I consider is a pregnant truth to your earnest attention because I anticipate the vastness of the fertile field it will surely occupy in the wonderful future which we may never see but which is sure to be.

Now come with me into the seductive realm of the enchantress Theory and reflect upon, debate and analyze the ideas that I have submitted to your attention. May not these in-

visibles, these atoms of Natural Creation, be the mysterious Absolute of our existence? Of our propagation and decease? Multitudes of these creatures toiling silently in the convoluted brain cells, like the maggots of odd Dean Swift's fancy, may create ideas, intellect, passions. Other legions, laboring in the blood corpuscles, may determine the purity or impurity of that precious medium and, thereby, its vitalizing or devitalizing virtues; while yet other hosts may control (or influence) the decay of our organic structures and their perishable framework? May not soils owe their fertility or sterility, their adaptability or impracticability to the unseen workings of these Genii of the Little? May not water, also, derive many of its properties, beneficial or injurious as they may happen to be, from its teeming contents of microscopical life? We already know (from Bruce, the traveller,) that the natives of far away Abyssinia are infested with parasite Worms through the agency of the water they drink—may not many diseases of the guts originate from germs more minute than these pernicious Larvæ?

All this I believe will be proven to be fact, and in its darker aspect we are very naturally

brought to see no hand of God. For what just, compassionate and merciful Father Diety would let loose a host of death dealing, ravenous Wolves in miniature, upon ignorant, frail, supplicating creatures of his kindly creation—his own, fore-known, fore-destined, children? But rather the astonishing, strange uncomprehended workings of some mightier power in almighty Nature infinitely beyond the weak, puny, priest-aborted rudiments of perception, dawning in the infant brain of Man: carrying out a grand design of unfathomable profundity, in which an atom is as mighty as a mass, a second as significant as a thousand years, and the smallest being in the universe of as much importance in the stupendous scheme as lordly Man, himself, with all his presumption, arrogance and self conceit thick upon him—leading us to exclaim with Hamlet, in immortal Shakespeare:

"There are more things in Heaven and Earth, Horatio,
Than are dreamt of in thy philosophy!"

FINIS.

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